CHEMISTRY IN AMERICA

EDGAR F. SMITH

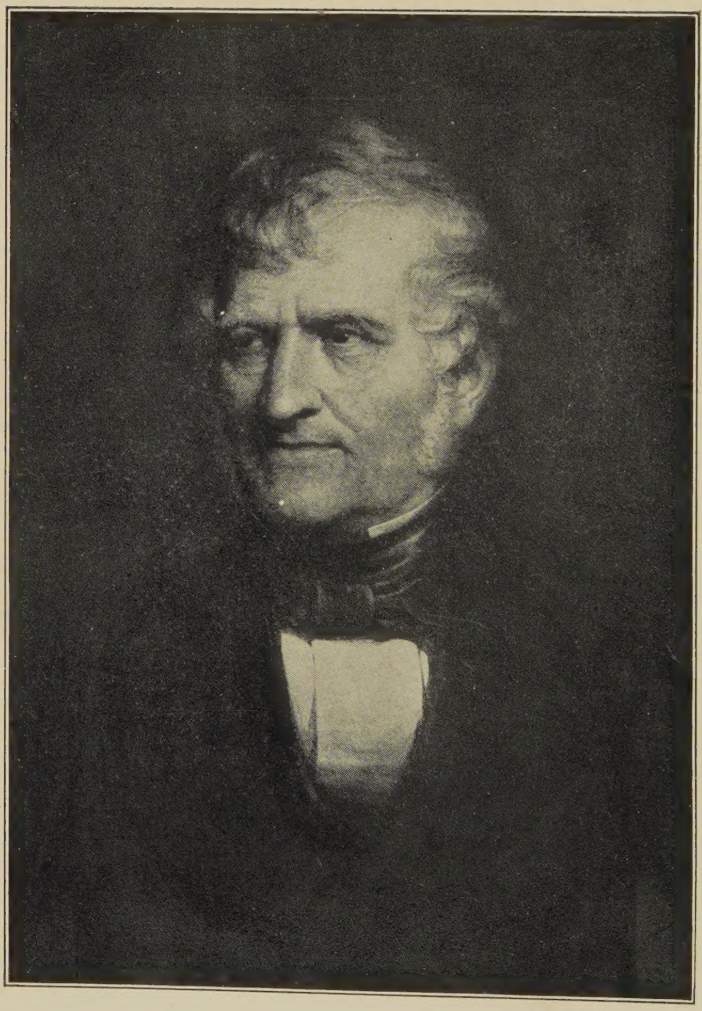
The Viking Library
VA. INSTITUTE OF TECHNOLOGY

The Vining Library
S. VA. INSTITUTE OF TECHNOLOGY

NEW RIVER STATE SCHOOL
LIBRARY
Division
Number

WITHDRAWN

CHEMISTRY IN AMERICA



Robert Hare

CHEMISTRY IN AMERICA

CHAPTERS FROM THE HISTORY OF THE SCIENCE IN THE UNITED STATES

BY

EDGAR F. SMITH

BLANCHARD PROFESSOR OF CHEMISTRY
UNIVERSITY OF PENNSYLVANIA

ILLUSTRATED

D. APPLETON AND COMPANY
NEW YORK AND LONDON
MCMXIV

QD
18
U6
S6

COPYRIGHT, 1914, BY
D. APPLETON AND COMPANY

Printed in the United States of America

THIS BOOK IS
AFFECTIONATELY DEDICATED
TO MY STUDENTS

529

PREFACE

The writer has lectured for several years to his graduate students on the development of Chemistry in the United States. A mass of material has been collected, most of which is not only interesting but valuable. Repeated requests have been made for the publication of these facts as a history of chemistry in the United States. To the writer's mind the information in his possession is not sufficiently complete to warrant such an important undertaking. The earliest endeavors of our country's scientists require even more careful and extended searching out.

Three addresses, delivered before the Chemical Society of Philadelphia, are included in the material collected by the author. These exhibit an earnest interest in chemistry in the early days of the Republic, and show, also, that despite unfavorable conditions our forefathers were eager to cultivate the science that promised so much for their country. Another proof of the genuine appreciation of the value of chemistry is evidenced by the founding of two Chemical Societies before a similar movement had been initiated anywhere else in the world. Minutes of these organizations have not been discovered. If found, they would surely contain most valuable information. Other papers, written during these early years, are even

now most difficult to procure, and as time passes will become more inaccessible.

These "Chapters" are submitted, then, that chemists of to-day may have, in easily accessible form, copies, at least, of some of the real treasures of our science, and with the hope that through them they may be stimulated to search for other, still hidden, documents of equal or of greater value. And, as incentives to the highest endeavors, there have been included brief sketches of the life histories of such men as Wolcott Gibbs, F. A. Genth, J. Lawrence Smith, Lea, Cooke, Willard Gibbs and others who have enriched the science of chemistry in this country, and whose names are still familiar to the student.

This compilation of chemical achievement in the United States has brought to the writer great joy, and a fund of helpful information, which he earnestly wishes may be shared by all who chance to read these pages.

In conclusion, the author's sincere thanks are extended to all persons and societies whose letters, books and documents have been drawn upon and used in this labor of love. He also gratefully acknowledges his indebtedness to his colleague—Professor Walter T. Taggart—for preparing the illustrations, which have been so successfully copied from old prints and photographs.

CONTENTS

CHAPTER I

PAGES

CONTRIBUTIONS OF AMERICANS TO CHEMISTRY.—Early Papers by de Normandie; by J. Madison; by M'Causlin 1-11

CHAPTER II

THE CHEMICAL SOCIETY OF PHILADELPHIA.—Its Formation.—Thomas P. Smith's Oration before the Society in 1798: "A Sketch of the Revolutions in Chemistry."—Women Participators in Chemistry.—Columbian Mineral.—Obituary of Thomas P. Smith 12-43

CHAPTER III

THE CHEMICAL SOCIETY OF PHILADELPHIA, CONTINUED.—Officers of the Society.—Advertisements of the Society.—Oration by Pascalis in 1802 44-75

CHAPTER IV

JAMES WOODHOUSE.—Woodhouse's "Chemical Catechism."—"The Young Chemist's Pocket Companion" (1797).—Pages from the Same.—Woodhouse's Answer to Priestley's Arguments against the Antiphlogistic System of Chemistry: Calcination of Metals in Air; Fixed Air;

CONTENTS

PAGES

Finery Cinder; Precipitation of One Metal by Another; Air in Charcoal.—Woodhouse's Letter to Maclean.—Methods of Obtaining Oxygenous Gas.—Silliman's Word-Picture of Woodhouse.—Rush on Woodhouse.—Caldwell on Woodhouse 76-108

CHAPTER V

JOSEPH PRIESTLEY.—Priestley's Arrival in America.—His Letters to Rush.—Minute of the Trustees of the University of Pennsylvania.—Priestley's Unpublished Letter to Dr. Mitchill (*facsimile*).—Description of Priestley by Silliman.—Sketch of Priestley by Caldwell.—Obituaries of Priestley 109-127

CHAPTER VI

THOMAS COOPER.—His Professorship in Dickinson College and in the University of Pennsylvania.—Decomposition of Potash with the Discovery of Potassium.—Cooper's Letter to Dr. Manners.—Sketch of the Apparatus Used in Isolating Potassium.—Woodhouse and Potassium.—Cooper and Hare.—Cooper and the Possible Origin of a Lincoln Phrase.—Caldwell's Estimate of Cooper.—Comparison of Priestley with Cooper.—"The Emporium of Arts and Sciences" 128-146

CHAPTER VII

JOHN MACLEAN AT PRINCETON.—Silliman's Estimate of Maclean.—Contemporaries of Maclean: Hutchinson, Dexter, Mitchill, Adam Seybert, Henry Seybert . 147-151

CONTENTS

CHAPTER VIII

PAGES

ROBERT HARE.—The Invention of the Oxy-hydrogen Blow-Pipe.—Memoir on the Blow-Pipe before the Chemical Society in 1802: Application of the Blow-Pipe; Account of the Manner and Principle of Its Action; Heat of Oxy-hydrogen Flame and Its Effect on Refractory Substances.—Silliman and Hare's Blow-Pipe.—Silliman's Description of Life in Philadelphia.—Silliman's Account of Hare.—Dr. Clarke and Hare's Blow-Pipe.—Hare's Deflagrator.—Hare's Calorimotor.—A Letter from Hare to Silliman.—Hare and Galvanism.—Amalgams.—Mercury as Cathode.—Electrolytic Process.—Deflagrating of Carburets 152-205

CHAPTER IX

SILLIMAN'S SECOND VISIT TO PHILADELPHIA.—COLUMBIAN CHEMICAL SOCIETY FOUNDED IN 1811.—Memoirs of the Society, Volume I: Preface; Papers before the Society; Officers and Members 206-218

CHAPTER X

LEADING CHEMISTS AS MINERALOGISTS.—Archibald Bruce.—Clemson.—Vanuxem.—Torrey.—Cloud. — Cleaveland.—Gorham.—James F. Dana.—Samuel L. Dana.—Bowen.—Troost.—Olmsted.—J. Redman Coxe and Electric Telegraphy. — Cutbush. — Emmett.—Griscom.—McNevin and the Atomic Theory.—W. W. Mather and the First Atomic Weight Determination Made in the United States.—Lewis C. Beck.—J. W. Bailey and the Common Blow-Pipe.—Samuel Guthrie and the Discovery of Chloroform.—Alexander Dallas Bache and the Specific

PAGES

Heat of the Atoms of Bodies.—John P. Norton and Agricultural Chemistry.—Evan Pugh and the Assimilation of Free or Uncombined Oxygen; His Presidency of the Pennsylvania State College.—Charles M. Wetherill.—The Rogers Brothers; Robert E. Rogers and Gas Diffusion; the Rogers' Wet Method for the Determination of Carbon.—Wormley's "Micro-Chemistry of Poisons" 219-244

CHAPTER XI

Other Eminent Chemists.—James C. Booth's Laboratory.—T. Sterry Hunt's Chemical, Mineralogical and Geological Investigations.—J. Lawrence Smith's Contributions to the Chemistry of Minerals and Meteorites.—Gibbs, Genth and J. Lawrence Smith.—The Cobaltamines.—Mineral Alterations 245-263

CHAPTER XII

Eminent Chemists, Continued.—Wolcott Gibbs; the Ammonia Cobalt Bases; Platinum Metals.—Complex Inorganic Acids; Electro-Analysis.—Prescott and Pharmaceutical Chemistry.—Johnson and Agricultural Chemistry.—Mallett and Atomic Weight Determinations.—M. Carey Lea and Photo-Chemistry.—Josiah P. Cooke and Atomic Weight Determinations; "The Numerical Relation Between the Atomic Weights."—J. Willard Gibbs and Physical Chemistry; the "Phase Rule"; Thermodynamics; Geometrical Methods; Chemically Homogeneous Substances.—Chemical Publications in the United States 264-353

INDEX 351

LIST OF ILLUSTRATIONS

	PAGE
Robert Hare	*Frontispiece*
James Woodhouse	76
Joseph Priestley	110
Priestley's Home and Laboratory	116
Thomas Cooper	128
Apparatus for the Decomposition of Potash	135
John Maclean	148
Samuel Latham Mitchill	150
Hare's Chemical Laboratory	158
Hare's Hydrostatic Balance	164
Hare's Deflagrator	188
Hare's Calorimotor	189
Hare's Apparatus for Obtaining Amalgams	194
A Section of Hare's Amalgam Crucible	198
Hare's Distillatory Apparatus	199
Hare's Apparatus for Deflagrating Carburets	202
Benjamin Silliman	206
James C. Booth	246
T. Sterry Hunt	252
J. Lawrence Smith	258
Wolcott Gibbs	264
M. Carey Lea	278
Josiah Parsons Cooke	302
J. Willard Gibbs	344

CHEMISTRY IN AMERICA

CHAPTER I

A REVIEW of the contributions of Americans to the Science of Chemistry would be incomplete without a consideration of the publications made in the earliest days in which scientific matters began to interest the people of our country. Before the United States became a republic, interest in such matters was manifested. The members of the venerable American Philosophical Society were most anxious to foster investigations along all lines of scientific endeavor. This is evidenced in a preface to the first volume of the Transactions of that Society, published in 1789, in which the aims of the Society were set forth—to seek the best methods of promoting the fertility of land and of protecting trees and plants from worms and insects, to improve useful animals, to preserve timber, and to ascertain the virtues and use of many plants; and, further, the Society expressly states that it will not confine its efforts wholly to these things, nor will it exclude other useful subjects such as Physics and Chemistry. "It is believed that the study of natural objects would have a tendency to inspire our youth with a love of knowledge, draw them gently from scenes of dissipation, and animate them with a laudable desire of distinguishing

themselves in the arts and sciences, by making useful discoveries that would honor them and promote the interests of the country." It is not surprising, then, that the earliest chemical contribution from this country, bearing the date September 10, 1768, appears on the pages of the Transactions of this Society under the title, "An Analysis of the Chalybeate Waters of Bristol in Pennsylvania," by Dr. John de Normandie, in which the author remarks that "although it must be confessed that a chymical analysis is, in some measure, an uncertain test of the medical virtues of any compound; and that the qualities of its constituent parts, when separated, may not only differ from, but are sometimes opposite to, those of the mixture; yet, when we want the testimony of experience, a chymical analysis is the best means of investigating the truth." He then proceeds to describe his experiences in the analysis of the water:

Experiment I. A small portion of white oak bark, infused in the waters, induced an immediate change from transparency to a dark purple colour, which it retained twenty-four hours, without depositing any sediment.

II. Some of the same water, after being made hot, or exposed for a few hours to the open air, in a great measure lost its irony taste, and received no other colour than a common tincture from the white oak bark.

III. One drop of strong oil of vitriol, in two ounces of the water, produced no sensible alteration; and the water after standing some time continues transparent, without depositing any okerish or other sediment to the sides or bottom.

IV. Ol. tart. pr. deliq. dropt in some of the same water, induced a change in the colour, rendering it somewhat

yellow; and in time precipitated to the bottom of the cup a fine gold coloured oker.

V. Sixteen ounces avoirdupois, carefully evaporated to a dryness in a China bowl in B. M. left one grain of a yellowish brown powder of the taste of tart. tartariz.

VI. Linen, moistened with the scum floating on the top of the spring, is tinged with a strong iron mold.

VII. This water in weight is exactly the same as that of rain water.

It is evident from these experiments that Dr. de Normandie had recourse to the use of the balance. What conclusion did the author draw from his investigations? He writes: "It is sufficiently evident that this water, in its natural state, contains a large portion of iron dissolved in pure water by means of an acid, which acid is extremely volatile, and probably of the vitriolic kind."

In a communication bearing a later date than the preceding, but published in the same Transactions, he adds some additional facts concerning the same water:

I. Upon the addition of Sp. Sal. Arom. to the water a slight effervescence ensued, and upon standing about an hour, a light yellow matter was separated and floated upon the top of the liquor.

II. From a mixture of lime water, the same separation was made, but fell to the bottom of the liquor.

III. Powder'd chalk added to the water produced the same separation, but not in so short a time as in the preceding experiments.

IV. The residuum, after a slight calcination, was strongly attracted by the magnet.

V. A solution of crude Sal. Ammon. being mixed with the water, was succeeded by the same appearance as the addition of lime water.

VI. The residuum after evaporation in Baln. Mar. before calcination, discovered to the taste a considerable portion of salt, which left a coldness on the tongue, and when separated by solution, filtration and evaporation, appeared of the colour of salt of amber, and shot into right angled crystals, which through a microscope appeared beautifully feathered; and from every experiment was found perfectly neutral.

VII. Silver immersed for some time in the water acquired a slight yellow colour.

VIII. The residuum thrown on a red hot iron sparkled very much, and emitted a sulphureous smell, what remained on the iron had not the least perceptible taste of salt.

IX. The waters, and the solution of the crystallized salt, changed syrup of violets to a fine light green.

The first four of these experiments, in which the waters were decomposed as well by a volatile alkali, as by lime water, and an absorbent earth, and the residuum (after a slight calcination) being attracted by the magnet, evidently prove that they are impregnated with a considerable portion of iron.

The fifth experiment (in which a decomposition takes place by means of a double elective attraction) shews that the acid in these waters has a stronger affinity with alkalies than that which is the basis of Sal. Ammoniac (which is the marine acid), and must be either the nitrous or vitriolic. And from a decomposition taking place, on the addition of common nitre with the Chalybeate waters, in about the same time as when left exposed in the open air, we may rationally conclude the acid to be of the vitriolic kind.

The sixth experiment shews that there is a small portion of neutral salts in these waters, which from the coldness with which they affect the tongue, and the appearance of the crystals, are probably of the ammoniacal kind.

The seventh and eighth experiments together with the smell of the bath and the considerable odour which the waters acquire when kept for any time, evidently shew that they contain a third principle, which is sulphur. This, indeed (as well as the salt), is in a small quantity, yet it may contribute somewhat to the medicinal virtues of these springs.

The ninth experiment seems to prove them to tend rather to an alkaline nature, but as this was in a very trifling degree, it may be accounted for from the escape of the acid which is extremely volatile.

These experiments compared with those I have already communicated to you sufficiently discover the constituent parts of these waters.

Somewhat later, in the second volume of the Transactions of the Philosophical Society, appeared a letter from J. Madison, Esq., to the astronomer, Rittenhouse, recording certain experiments "upon what are commonly called the Sweet Springs."

Experiment I. Having plunged a very sensible mercurial thermometer in the spring, it stood at 73°. The temperature of the air was about 69°.

II. A good hydrometer sunk one-twentieth of an inch deeper in common mountain water than in the spring.

III. Nut-galls mixed with the water in a wine glass struck a palish brown, which shewed that there was little or no iron in it.

IV. Violets, mixed with the water in a wine glass, turned it in a short time of a reddish colour. This was a proof that the waters contained some kind of an acid.

V. Having made a solution of silver in the nitrous acid, and mixed a little of it with the water, it immediately became milky, and a white pulverulent precipitate ensued.

The experiment shewed, by the whiteness of the precipitate, that the waters contained nothing sulphureous, and by the pulverulency of the precipitate that the acid contained in the waters was vitriolic.

VI. A solution of lead in the nitrous acid being mixed with the water, it becomes somewhat milky, and a white precipitate was observed. This experiment also shews that the waters contain an acid, most probably the vitriolic, and also that they contain calcareous earth. Soap is not readily miscible with them.

VII. A solution of saccharum saturni in the nitrous acid being made, and lines marked upon paper with it, and placed over the water, the lines retained their former colour. This experiment also shews that the water contains nothing sulphureous.

VIII. Having poured a little of the spirit of salt into the water, after some time a coloured precipitate was observed, but as the waters did not strike a green or blue colour, it shewed that there was no copper in them.

IX. A solution of vitriol of copper mixed with the water produced a thick, green, curdly appearance, but did not become bluer. This experiment shewed that there was no vol. alkali contained in them.

X. The vitriolic acid mixed with the water suddenly effervesced, and produced a heat which raised the thermometer from 75° to 83°, by applying the bulb to the outside of the glass.

As the spring is continually discharging large bubbles of air, which rising from the bottom break upon the surface of the water, I was desirous of making some experiments upon the air, in order to determine whether the acidity of the water might not be owing to it; and also to determine the nature of the air, whether fixed or not. Having therefore caught a quantity of the air in a decanter, I communicated a part of it to an equal bulk of pure mountain water, and after agitating them for some time, gave

it to several to taste; who agreed that it had the taste of the spring water. Upon a second trial this experiment did not succeed. I had not an opportunity of trying the nature of the air by means of chalk-water, and was prevented from prosecuting any farther inquiries into the nature of these celebrated waters by a sudden alarm, to which the frontiers were then continually exposed.

These waters have been falsely called sweet, for their taste is evidently acidulous. The experiments also shew that they contain an acid. Their taste resembles exactly that of waters artificially impregnated with fixed air, extricated from chalk, by means of the vitriolic acid, and I conceive must be nearly the same with the true Pyrmont water. They have little or no smell, do not form an incrustation, nor do they leave a deposit upon standing many hours. Upon bathing in the morning, the skin has a soapy kind of feel. This was not observed in the evening.

There is near this spring another, a very strong chalybeate.

In 1789, Dr. Robert M'Causlin communicated "An Account of an Earthy Substance found near the Falls of Niagara and vulgarly called the Spray of the Falls," which contains the following record of experiments:

Experiment 1. I put an opaque piece, weighing 14 grains, into the vitriolic acid diluted with three times its quantity of water; and let it remain there twenty-four hours, shaking it now and then. Not the least effervescence ensued, and on taking out the piece it weighed near one grain more than when it was put in, although care was taken to absorb the moisture which was upon its surface. This experiment was repeated with a shining piece, and with exactly the same results.

Exp. 2nd. When put into vinegar it did not produce the

least effervescence. The vinegar having stood upon it some time was then poured off and spirit of vitriol dropped into it, yet not the least precipitation ensued.

That I might not be led into error by the vinegar not being good of its kind, I repeated these experiments with chalk; and as both effervescence and precipitation took place it was evident that there was no defect in the vinegar.

Exp. 3rd. A small piece was exposed to the heat of a blacksmith's forge during fifteen hours. Upon taking it out and pouring water upon it, no ebullition ensued: nevertheless it tasted like weak lime water; being then divided into two portions, a solution of mild fixed alkali was dropped into the first, and immediately a precipitation ensued. The second portion being exposed to the air in a tea-cup soon contracted a changeable coloured film, which next morning was become very thick, resembling in every respect that of lime water.

Exp. 4th. Hot water being poured upon some of these substances, reduced to powder and the whole suffered to settle, the clear liquor had not the taste of lime water as in the third experiment; nevertheless a solution of mild fixed alkali being dropped into it as copious a precipitation ensued as when the earth had undergone calcination.

As I had neither the nitrous nor muriatic acids, nor even caustic fixed alkali, I had it not in my power to make any trials with them.

From these experiments we may, perhaps, be authorized to draw the following conclusions:

I. That this concrete is not an alkaline earth, as it is not affected either by the vitriolic or vegetable acids.

II. We may, with more probability, say that it is a combination of an acid with a calcareous earth, and that it might with propriety be ranked amongst the selenites. This supposition is founded upon the following reasons: 1st, it appears from the fourth experiment that it is par-

tially soluble in water, and that its earth can be precipitated by a mild fixed alkali: 2ndly, the third experiment shews evidently that its earth is of the calcareous kind, as appears by the styptic taste and changeable coloured film, agreeing exactly with common lime water. It seems probable that the vehemence of the fire had in part expelled the acid, leaving a portion of the mass in the state of quicklime.—It is well known that most waters are more or less impregnated with a selenitic matter. It is said that agitation disposes water to deposit a part of its earth.

It is also agreed that water becomes more pure by being freed from its earthy parts.

These three considerations, together with the result of the above experiment, inclined me much to favour an opinion which universally prevails in this part of the world, viz.: That the water is purified in coming down the Falls. They also suggested a thought to me, that this purification might depend upon the latter depositing part of its earth in consequence of the violent agitation it had received in passing over rapids upwards of a mile in length, and the tumbling down the falls. Such a supposition received great support from the substance called the Spray being only found at the bottom of the Falls, which seemed to show that a deposition did actually take place. This theory was very plausible, and gave me, at first, much pleasure in contemplating it: Nevertheless succeeding observations and more strict inquiries have led me to entertain many doubts upon the subject.—That the water is much better at Niagara, which is about thirteen or fourteen miles below the Falls, than it is at Fort Schlosser, which is about a mile and a half above them, is an unquestionable fact: Nevertheless, I do not think that this can with strict justice be alone attributed to the deposition of the earthy parts. There are several low marshy grounds which empty themselves by small creeks into the river immediately above the Falls, and it is reasonable to suppose that such an

impregnation would be more sensibly perceived at its source than afterwards, when it is mixed and diluted with the water of the river. To this may be added that at Fort Erie, about twenty miles above the Falls, the water is thought not to be inferior to that of Niagara. In the second place, it occurred to me, that if any considerable deposition of earth took place, as I had supposed, the specific gravity of the water below the Falls must be less than that of the water above. To determine this point, I weighed a quantity of water at Niagara with all the care and exactness I was master of, and the very same day made a journey up to Fort Schlosser, and weighed the water immediately above the Falls. The specific gravity was found to be exactly the same. As I conducted this experiment with all possible caution, measuring the temperature of the water and also that of the room, in which it was weighed, each time by Fahrenheit's thermometer, I think I can depend upon its being pretty accurate. In inquiring into the formation of this substance called the Spray, it must be observed: 1st, that the rocks near the Falls are kept constantly wet by the vapour which rises in form of a thick mist; and even those at a distance of a quarter of a mile, or more, are affected by it, when the wind blows down the river; 2ndly, that these rocks, either from the nature of their structure, or from the circumstance of their being kept constantly wet; or perhaps from the Spray accumulating between their layers, and acting as a wedge, are very apt to crack and split; and hence are almost constantly tumbling down in larger or smaller pieces; 3rdly, that upon separating the layers of these rocks there is generally more or less of this substance called Spray found between them and almost universally in a soft state. From the best inquiries I have been able to make, during a residence of many years, this substance is never found above the Falls, perhaps never at a much greater distance than one mile below them. Close to the

Falls it is found between the layers of most of the rocks, the quantity lessening in proportion to the distance from the Falls. Upon comparing all these circumstances together, it seems probable that this substance is formed by the moisture arising from the Falls constantly and slowly filtering between the layers of the rocks, and it seems very possible that the violent agitation which the water has undergone may dispose it to part with earth more easily than it otherwise could do.—The circumstance of the Spray not being found above the Falls seems to suggest an opinion that that part of the vapour which hangs upon the surrounding rocks is the heaviest as being most loaded with earthy particles, whilst the remainder which mounts up is the purest and contains little or no earth. The want of proper rocks to filter through and to attract the earthy particles may likewise be a reason why the Spray is not found above the Falls, and the specific gravity of the water which runs down the channel of the river below the Falls being equal to that above them (which seems to argue that from want of some attracting body it had parted with little or none of its earth) favours such a supposition.

These communications testify to a spirit of inquiry, at least, on the part of our early devotees to science. They are, further, interesting in that they show the use of the balance as early as 1768 and indicate the steps of analysis, always regarded as of prime importance.

CHAPTER II

MODERN chemistry began when Lavoisier overthrew the views of Stahl and presented his own anti-phlogiston theory. It is interesting to note that the thought of the French School reached this country very early and there were here those who defended it.

Chief among these was James Woodhouse, who had founded the Chemical Society of Philadelphia in 1792. This was the first chemical society in the world. As far as can be learned, Woodhouse was its first and only president. This society lived about seventeen years. Its members favored Lavoisier's doctrine of combustion. The minutes of the society have never been found, although diligent search has been made for them. There appear to have been two classes of membership—regular and junior. On the title pages of many of the theses offered by the students of the Medical Department of the University of Pennsylvania will be noticed, after the name of the author, "Junior member of the Chemical Society of Philadelphia."

Every year an address was delivered before the Society. If the latter continued seventeen years, there should be that many addresses, but it is doubtful whether any person living at the present time has seen more than three or four of them. As a matter of historical interest, three will be given in full:

ANNUAL ORATION

DELIVERED BEFORE THE

CHEMICAL SOCIETY
OF PHILADELPHIA,

April 11, 1798

A SKETCH OF THE REVOLUTIONS IN CHEMISTRY,

BY
THOMAS P. SMITH

PHILADELPHIA:

Printed by Samuel H. Smith,
No. 118 Chestnut Street

MDCCXCVIII

To ROBERT PATTERSON, A. M.

Professor of Mathematics in the University of Pennsylvania.

SIR:

I KNOW no person to whom my first essay can be DEDICATED with so much propriety as to the instructor of my early youth. Accept it then as a small, but sincere, tribute of gratitude, from

Your friend and pupil,
THOMAS P. SMITH.

PHILADELPHIA LABORATORY.

April 14th, 1798.

In meeting of the Chemical Society of Philadelphia.

RESOLVED—That a copy of Mr. Smith's learned and ingenious Oration be requested for publication.

Extract from the minutes,

GEORGE LEE,
Junior Secretary.

INTRODUCTION.

Gentlemen of the Chemical Society:

Having been honoured by you with the appointment to deliver the Annual Oration, I have, with diffidence, prepared myself to comply with your request. I shall not attempt to apologize for any imperfections it may contain, however numerous they may be, as they are the inevitable effects of your choice. But there is one liberty I have taken, for which I consider myself bound to apologize.

The *Resolution,* in pursuance of which this oration is delivered, directs that it shall contain all the discoveries made in the science of chemistry during the preceding year. Instead of complying with the *letter* of this resolution, I have taken the liberty of preparing for you a *sketch of the revolutions in chemistry.* To this I have been induced from a consideration of the utility and pleasure that always result from a knowledge of the origin of our opinions. He who should take up his abode on the banks of a stream, and quench his thirst from its waters, could not feel uninterested in a knowledge of its source, and the course it has run. Knowing over what substances its waters have passed, he is enabled in some measure to judge of their purity, and is put on his guard against any bad effects that may be produced by them. Thus, by knowing the origin of our opinions and the channels through which they have come to us, we can form a tolerable judgment of what particular prejudices they are most likely to be biassed by, and be thus put on our guard against receiving them without the strictest examination. Such were the reasons which induced me to write, and which I hope will induce you to pardon me for delivering before you, A Sketch of the Revolutions in Chemistry.

The origin of CHEMISTRY, like the origin of every other science that early dawned upon mankind, lies buried beneath the dark fables of antiquity. The ascription of the discovery of truths, or the invention of arts, beneficial to mankind, to supernatural beings, was so general during those dark ages of ignorance and superstition, that we are not to wonder that the science of chemistry was supposed to have had a divine origin.

If music, poetry, and painting; if the arts of making wine, raising grain, healing the sick, had their tutelary deities who were supposed to have taught them to man, if the Egyptian, when he beheld the Nile, without any apparent cause to him, who was ignorant of its source, periodically overflow its banks, fertilize his land, and then peaceably retire within its proper limits, supposed it to descend from heaven, should we not expect that chemistry, a science to which almost all others owe their birth, would have been supposed to have been derived from the GODS? Accordingly we find this to be the prevailing opinion among the ancients. But however interesting an investigation of these fables may be to such as imagine them allegorical accounts of the origin of chemistry, we must pass them over as the unmeaning offspring of IGNORANCE and SUPERSTITION.

Were we to endeavor to search out the true origin of chemistry, we should find ourselves bewildered to little or no purpose among the multifarious traditions of antiquity; like the traveller who should in vain attempt to ascertain the true source of a great river, formed by the union of a number of small streams, we should after much labour and disappointment give up the pursuit as one in which the effect produced would not repay us for the labour endured.

The Arabians appear to have been the first people who made any considerable progress in chemistry. For, however great the extent of knowledge in this science the

votaries of antiquity may ascribe to the Egyptians, we cannot consider them as having made any great progress in it. It is true they had carried on some of the practical parts of it, such as the working of metals, imitating precious stones, and painting on glass, to a considerable degree of perfection; yet they do not appear to have possessed any knowledge of its general principles. It was as yet confined to the forge of the *smith* and the workshop of the *lapidary;* and they expected their processes to terminate favourably only because their predecessors, who perhaps were taught by some happy accident, promised them success. Their priests, indeed, pretended to extensive knowledge in this as well as in every other science, but as they have left us no data by which we can judge of their knowledge, we are led to believe that in these pretensions, as well as to those in great sanctity, their object was merely to gain an ascendency over weak minds. The simplest ideas represented by their hieroglyphick characters were converted by the eye of IGNORANCE, who venerates everything she does not understand, into the most sublime truths. Hence, arose the idea that these priests, who perhaps understood little more than how to delude a superstitious, ignorant people, were possessed of a knowledge of all the arcana of nature.

Nor need we be surprised that this was the case in those dark ages, when even in this enlightened century men are found weak enough to spend their time in the solution of ancient fables, in search of truths which are only to be discovered by contemplating the works of nature, and who have the effrontery to declare that in these puerilities they can easily discern that the ancients were possessed of a knowledge of almost all that the moderns have thought themselves the discoverers. So far, indeed, has this blind attachment to antiquity been carried that it need not excite your surprise, if some of these *fabulous commentators,* more deeply learned than his fellow la-

bourers, should, by means of a smoking chalice found on the pillar of Trajan, transfer from Lavoisier to an Egyptian priest the honour of the pneumatic theory; or, by the bowl of the sacrifice overflowing with blood, painted on a Mummy, deprive PRIESTLEY of the honour of the discovery of the oxigination of the blood.

To the Arabians, then, is to be ascribed the honour of being the first nation in which chemistry ceased to be nothing more than a knowledge of a few processes in the arts confined to the work-shops of illiterate mechanics. To this nation we are indebted for its application to medicine which was first effected in the tenth century by Rhases, a physician of the hospital of Bagdad. It now became an object worthy the attention of men of letters and genius. The phænomena of nature were scrutinized with an attentive eye, new processes were instituted for determining her laws, and bodies before supposed *simple,* were analized by means of newly discovered agents. Such was the situation of chemistry in Arabia, when, by means apparently little favourable to the dissemination of science, it was transplanted into the west of Europe.

Towards the close of the eleventh century all *Christendom* was roused to arms by the declamation of an obscure individual of the name of Peter, sirnamed the Hermit. This man, who to the most barbarous ferocity added the most refined cunning, travelled over Europe preaching up a *croisade* to recover from the hands of the *infidels* the *holy*-land. As his hearers were plunged in the most barbarous ignorance, their passions were easily wrought upon, and this man who in the eighteenth century would be confined in a *mad-house,* or treated with contempt, in the eleventh raised an army of 700,000 men to effect his absurd scheme. This army of which BIGOTRY and SUPERSTITION led the van, and MURDER and RAPINE closed the rear, was composed of men of every rank and profession. And this army, however unfit a medium it may appear for the trans-

mission of science, was the means by which chemistry was first transplanted into Europe.

That men actuated by such motives as the croisaders were, who, with the symbols of peace in one hand, and a reeking sword in the other, marked their footsteps with the blood of women and children; who had left their peaceful habitations, their wives, their children and all the joys of domestic happiness, to enforce by the sword the truth of a religion whose basis is charity, and to wrest from the hands of the *infidels,* in another quarter of the world, a barren tract of land almost unfit for the habitation of man, because it was the birthplace of their religion; that these men should be the disseminators of science is a paradox in the history of the human mind that at first view appears inexplicable. But the difficulty vanishes when we recollect that, happily for the cause of science, the chemistry of the Arabians was deeply tinged with alchemical notions.*

The croisaders, who were blind to the charms of science, were far from being so to those of gold. As soon, therefore, as chance threw in their way pretenders to the art of converting the metals, their avarice was roused, and for the sake of the promised wealth they *condescended* to study chemistry. After the defeat of this immense army, many, who had set off with a design of converting the *infidels* to *Christianity* by means of the sword, returned to endeavor to convert all the metals into gold by means of their newly acquired chemical agents.

Europe soon swarmed with people in search of the agent, by means of which the baser metals were to be converted into gold and silver, and to which they had given the name of the Philosopher's stone. All classes of people were seized with the mania. The indigent man, who was instigated to study by the hopes of acquiring wealth, but who for want of money to commence his operations was unable

* See note A at the end.

to proceed, was sure to find a patron among the wealthy, who, upon the condition of sharing in the discovery, advanced large sums of money for carrying on the operations. Processes of the most extensive and expensive nature were instituted in search of this chimerical substance, and the most important discoveries, though different from that most hoped for, were made. No expense of labour or time was spared, and immense fortunes were dissipated by men who would not have advanced the smallest sum for the discovery of any truth whatever from which they could not hope to derive some pecuniary advantage. Thus was avarice enlisted in the cause of science, and thus that worst passion of the human breast, which has ever, but at this single period, retarded the progress of science, now tended in the most astonishing manner to its promotion.

That branch of chemistry called mineralogy particularly flourished during these researches. The metals were the objects to which the attention of the alchemists was immediately directed. Hence considerable progress was made in the art of extracting them from their ores and working them.

From an idea entertained by some of the alchemists that the philosopher's stone was to be the result of an intimate union of *sulphur* and *mercury*, this semi-metal became in a peculiar manner the object of their attention. The result was that the materia medica became enriched with many invaluable preparations of it.

This period gave birth to a number of men of the most respectable talents; at the head of these we must rank Roger Bacon, who flourished in the thirteenth century, and whose mind was deeply tinged with alchemical notions.

In the course of some chemical experiments, Bacon having mixed nitre, sulphur and charcoal together, in a mortar, they by accident took fire and produced a loud explosion; this first suggested to him the idea of making gunpowder, which, from a false idea he entertained of the

terrible effects that would be produced by its being generally known, he concealed in his writings under the form of an anagram.

In the sixteenth century a new sect of alchemists appeared, who were in search of a medicine that should cure all diseases.

The Arabians, in their treatises on alchemy, had employed that figurative language which is so universal in the East. The agents they used for bringing metals to *perfection* they called *medicines,* the *imperfect* metals *sick men,* and gold *a sound, lively, healthy, durable man.* When the Europeans procured translations of these works many of them understood all these figurative expressions in a literal sense, and when in the course of their reading they met with passages like the following from Geber, *"Gold thus prepared cures lepras, cures all diseases,"* and in which he only meant it would transmute all other metals into gold, they understood it to be a medicine by which all the diseases to which the human frame is liable might be cured.* Such was the origin of a sect of alchemists to whose industry we are indebted for the most valuable accessions to the materia medica.

At the head of this sect of alchemists stood Paracelsus, a name familiar to every chemist. He was born near Zurich in Switzerland, in 1493. From his earliest youth he seems to have possessed all that wildness of imagination which so strongly characterizes his countrymen. The moment he conceived a thing possible, he formed a theory for the performance of it, and then proclaimed to the world he had effected it. As soon, therefore, as he conceived of the possibility of forming a *panacea,* he commenced his search after it, and emboldened by the success of some of his mercurial preparations he declared he possessed the power of closing forever the door of the tomb.

Having likewise formed an idea of a liquor that should

* Boerhaave.

dissolve every substance in nature, and to which he gave the name of the *alcahest,* he declared to the world he had discovered it, and published a book in which he gave an account of many of its operations. This book abounds with the wildest extravagances and most palpable contradictions. In several passages of it he tells of his having dissolved various substances in the *alcahest,* in vessels hermetically sealed, in which operations, although the substances were readily dissolved, the vessels appear to have remained undissolved by this universal solvent. Van Helmont has likewise written much on the subject of the *alcahest,* and has the effrontery to declare that he possessed for a considerable time a vial containing this wonderful liquor, but that it was given to him, and afterwards taken away from him.

In addition to the panacea and alcahest, Paracelsus declared himself possessed of the philosopher's stone. Thus he persuaded the greater part of his contemporaries that he was possessed of what they conceived the two greatest blessings man can enjoy, the unlimited power of increasing his wealth and prolonging his life. Against the truth of an opinion so generally entertained by his contemporaries we shall offer but one objection. Paracelsus at the latter end of his life wandered about Europe in poverty, and died at the age of forty-eight, to the disgrace of his boasted aurum potabile, azophs, little demons, elixirs, and immortal catholicons, after a few days' sickness at a public inn at Saltzburg, although he had flattered himself that by the use of his *elixer proprietatis* he should live as long as Methuselah.

The failure of Paracelsus did not intimidate others from pursuing the chimera, among the number, Cassius, known by his precipitate of gold; Libavius, whose name is affixed to a preparation of tin; Sir Kenelm Digby, who believed in the sympathetic action of medicaments; Van Helmont, famous for his medical opinions and chemical notions; and

Borrichius, a Danish chemist, who first discovered the method of inflaming the oils by nitric acid, are particularly to be noticed for their talents. To the labours of these men we are indebted for many valuable medicines. But in a peculiar manner they demand our gratitude for the intimate union they have produced between medicine and chemistry, the consequence of which has been that disease has been stripped of half its terrors.

Amid the dark, gathering clouds of ignorance and superstition, that hung over all Europe during this century, one ray of light burst forth so pure and strong as to indicate a rapid dispersion of the worse than Egyptian darkness of the age. Francis Bacon, a name that must at once draw forth our pity and admiration, appeared at the latter end of the sixteenth century, and laid the foundation of natural philosophy on the true and immutable basis of reason. For some centuries past the world had been engaged not in discussing philosophical truths, but the opinions of philosophers. The book of Nature, from whence alone true knowledge can be drawn, was entirely neglected, and the works of Aristotle and Plato were made use of to supply its place. Instead of endeavoring to discover her laws by observing their effects, they attempted to explain them by the *categories* of the Peripatetics or the ideas of the Platonists. But Bacon, perceiving that these *ignes fatui* only served to lead astray, chose for his guide the invariable light of reason. By this he soon perceived that a knowledge of the laws of Nature can only be acquired by observing her operations. He therefore advised mankind, instead of spending their time in interpreting the idle dreams of mystical philosophers, to forsake their air-built castles and by experiment erect their systems on the adamantine basis of truth. Not content, like the generality of reformers, with barely pointing out the road they ought to pursue, this able pioneer proceeded a considerable distance in it, and cleared the way to many of

the greatest discoveries of his successors. But either from that love of unintelligible systems which is so common in ignorant men, or from a dread of entering on a new road, the termination of which they could not perceive, it was some time before mankind could be drawn from the beaten track.

About this time, Glauber, a German, rendered essential service to chemistry, by examining the residues of operations which had heretofore been thrown aside as useless and distinguished by the names of *caput mortuum,* or, *terra damnata.* By this he discovered the *sulphate of soda,* called after him Glauber's salts, and the *sulphate of ammoniac;* and threw great light on the processes for preparing mineral acids.

At the commencement of the seventeenth century the alchemical mania arrived at its acme. In Germany a society was formed under the name of the Rosicrucians, of which little more is known than that they pretended to be in possession of the secrets of transmutation, of the universal sciences and medicine, with the science of occult things. In France, England, Spain, and most of the other nations of Europe, the belief in alchemy was carried so far that decrees were issued by government forbidding it to be practiced, lest the value of the current coin of the nation should be destroyed, or individuals practicing it be rendered too powerful.

This triumph of error over reason, like every other triumph of the same nature, was, however, doomed to have an end. Father Kircher, a Jesuit, author of a great work entitled *"Mundus Subterraneus,"* and Corringuis, a learned physician, commenced an attack on it, which, by the aid of the philosophical chemists who made their appearance some years after, totally destroyed this chemical monster.

Chemistry has hitherto consisted of a multitude of facts, disseminated without any regard to arrangement over many loose dissertations on its various objects. "As yet,"

as Macquer observes, "there were many branches of chemistry in being, though the science itself was not yet in existence." Towards the middle of the seventeenth century James Barnet, physician to the king of Poland, first collected and arranged the principal known facts in a methodical manner, and added observations thereon. Bohnius, professor at Leipsic, likewise formed a methodical collection. But Joachim Becher of Spires wrote a work entitled *"Physica Subterranea,"* which from the precision with which the facts are related, and his observations on them, so far eclipsed the writings of Barnet and Bohnius that their works are now totally neglected, and their names almost forgotten. This work, in which there are a number of conjectures verified by late discoveries, had the honour of having for a commentator one of the brightest ornaments chemistry can boast—I mean the celebrated STAHL.

George Ernest Stahl was born at Onold in Franconia, in 1660. From his earliest youth he appears to have been attached to the study of chemistry. But in a particular manner his mind was directed to ascertain the true principle of inflammability.

Until the time of Becher, the most vague notions were entertained on this subject, the chemists supposing it to be a *sulphur* pervading all inflammable bodies. Becher, perceiving that sulphur did not exist in many animal and vegetable substances, although inflammable, asserted that it was not the principle of inflammability, but that this principle resided in a substance common to sulphur as well as all other inflammable bodies; this substance he supposed to be of a *dry* nature, and therefore called it an *earth,* and to distinguish it from all other earths, he called it PHLOGISTON. This doctrine was adopted by Stahl, who so far improved and extended it, that he is now generally considered as its founder.

The human mind delights in speculative reasoning. It

can scarcely receive two connected facts without wishing to draw a general conclusion. It is this spirit of generalization which has given birth to some of the most sublime as well as the wildest theories. Without it the mind of man would be nothing better than a wild chaos of facts. Instead of a well constructed temple, throughout the whole of which reigns the most perfect harmony, it would be a mere quarry in which although all the materials for constructing the temple are contained, yet they are in so rude and deranged a state that they are neither useful nor elegant. The assent of mankind, it is true, is often obtained to the wildest theories; yet even these, false as they may be, serve the purpose of giving some sort of arrangement to all the known facts, and any arrangement, however bad, is better than none. Nor need we be afraid that any false theory, however specious it may appear, will be permanent; for, WHATEVER SYSTEM IS NOT FOUNDED IN TRUTH MUST FALL!

As soon then as Becher's doctrine of phlogiston, as improved by Stahl, became generally known, it was adopted by the undissenting voice of the chemical world. It answered for the limited state of chemical knowledge, and the philosophers from his time until within a few years past knew of no phænomena in combustion that they could not account for satisfactorily, *to themselves,* by this theory.

About this time lived Mayow, an English physician, famous for a number of *ingenious conjectures.* According to Dr. Haller, he supposed that *nitre* floating in the air was absorbed into the lungs and formed the *animal spirits,* the heat of the system, and imparted colour to the blood. Blumenbach says he was one of the first authors who wrote concerning the factitious airs, especially that now called *dephlogisticated air,* or *oxigene;* the *"spiritus nitri ærius"* of Mayow. The work which contains his peculiar notions on this subject is entitled *"Tractatus duo: de*

respiratione, et de rachitide," published at Oxford in 1668. He was but thirty-four years of age when he died.

We are now entering on the most brilliant era that has ever occurred in this science. Hitherto the progress of chemistry had been slow and uncertain. It depended on accidental discoveries made in search of chimerical objects. Its votaries were not led on so much by the love of truth as the love of life and wealth. But the theory of Becher and Stahl gave a new direction to the pursuits of chemists, and instead of the philosopher's stone, alcahest and panacea, their labours were now directed to the establishment of a theory of combustion.

Stahl, whose mind was entirely occupied with demonstrating his favorite theory, and observing all the supposed modifications of phlogiston, seems to have overlooked the influence of air in all the phænomena which he attributes to his inflammable principle. The necessity of attending to this fluid in the operations of chemistry had already been demonstrated by Boyle and Hales. The difference between chemical events that happen in like circumstances in air and in vacuo had been observed by the former and the latter had procured from various substances different kinds of air. He thought air was the cause of solidity in bodies.

Dr. Priestley, in pursuing the experiments of Hales, discovered many elastic fluids which had heretofore been entirely overlooked by the chemists. Dr. Hales had obtained air from *minium,* but he had not investigated its properties. On the first of August, 1774, a day which will ever be conspicuous in the annals of science, Priestley obtained this air,* and found it much purer than atmospheric air. In the course of some experiments he instituted on this air, he found it to be the cause of the red colour ac-

* Called by him *dephlogisticated air,* from his supposing it to be air deprived of all phlogiston, and by the French chemists *oxigene,* from its being the principle of acidification.

quired by the blood in passing through the lungs. This discovery has laid the foundation of a theory of animal heat that has thrown more light on the science of physiology than perhaps any other discovery which has ever been made.*

Mr. Lavoisier soon after proved that the weight acquired by heated bodies is owing to an absorption of oxigene.† To this discovery we are indebted for the French system of chemistry.

Before entering on this revolution, the greatest perhaps that has ever occurred in this or any other science, you will pardon me for occupying a few minutes of your time in paying the debt of gratitude we owe to him by whom it was effected.

Lavoisier was born at Paris, August 16th, 1743. From his earliest youth he maintained a genius of no common order. At the age of three and twenty he obtained from the Academy of Sciences a gold medal for a dissertation on the best mode of enlightening during the night the streets of a great city. Two years afterwards he was made a member of that justly celebrated society. As yet his mind was confined to no particular branch of science, but each in its turn was benefited by his attention. Until at length, about 1770, Lavoisier, struck with the importance of the discoveries which had recently been made by Priestley, Black,‡ Cavendish, and Macbride, relative to elastic fluids, turned his attention to this inexhaustible source of discovery.

He had now entered on a career which was to rank his name with those of Bacon, Newton and Hartley.

* See note on combustion.

† See note B.

‡ In 1775 Dr. Black discovered *fixed air* or the *carbonic acid* in *calcareous earth.* He affirmed that the dissipation of this air converts it into *lime,* and that, by restoring it again to the *lime, calcareous earth* is regenerated.

His time and fortune were devoted to furthering discoveries in chemistry, and his house became a great laboratory filled with every species of apparatus necessary in this science. Here he made welcome men of science to whatever nation they might belong, or to whatever opinions they might be attached. Twice a week he held assemblies at his house, to which was invited every person most eminent in geometrical or physical knowledge. Here all the new chemical opinions which appeared in Europe were discussed and tested by experiment. Before this assembly Lavoisier tried all his experiments, and listened with candour to the discussion of them. To this line of proceeding we are indebted for that accuracy of experimenting, which has been introduced, instead of the former incorrect mode. After his experiments and theories had passed this strict ordeal, and not before, he gave them to the world.

It is to these assemblies we are indebted for the new nomenclature, which the French chemists have introduced into this science.* This nomenclature has tended considerably, by banishing much of the technical jargon of chemistry, to its promotion, and leaves nothing for us to wish, but that they who made us so happy a commencement had extended it still farther. We may consider it as a happy omen of what we are to expect from an introduction of a philosophical language into the sciences.

The effects of these labours of Lavoisier are to be found in forty memoirs, replete with the grandest ideas relative to the various phænomena of chemistry, published by him, from the year 1772 to 1793, in the transactions of the French Academy. In 1784, he formed an idea of collecting into a single work all the discoveries he had given to the world at different periods. This work, which did not appear until 1789, exhibited the simplicity of his system in

* See note C.

so forcible a point of view that it soon gained the almost universal suffrage of the chemical world.

Hitherto we have beheld Lavoisier only as the philosopher, rending the veil of nature, and drawing into view all her native charms. Let us now view him in the no less exalted station of private life. If, as philosopher, he raises our astonishment by the brilliancy of his discoveries and profundity of his reasonings; as a man he no less excites our admiration by his strict performance of all the duties of a friend, a relative, and a citizen. In short, Lavoisier was one of those truly exalted characters that prove the folly of the observation, made by *malicious ignorance,* that a love of science and a performance of the duties of life are incompatible.

Our picture has as yet displayed none but the most pleasing colouring.—Would to heaven I could, consistently with my duty, put it out of my hands, unfinished as it is. But there is one dark shade, which, to complete it, must be laid in, and which will efface the pleasure arising from a contemplation of its beauties.

Lavoisier was strongly attached to the cause of SCIENCE AND TRUTH, and consequently of that of LIBERTY. When the French revolution burst forth on the astonished world, he, therefore, early appeared as its advocate. Until at length Robespierre, having *descended* from the elevated station of a Representative of the People, to the debased one of their Tyrant, perceiving that a love of science and truth naturally produced a love of liberty, determined on the destruction of all those who united these dangerous qualities. Lavoisier was one among the many marked out for destruction. No other excuse could be found for his execution, than that he had been a *farmer-general* under the old government. But this excuse, weak as it was, was sufficient for the *tyrant,* who had the *power* and the *will* to destroy him. Let us draw a veil over the fatal catastrophe that has deprived the republic of science of its

brightest ornament.* And while we mourne the loss of this benefactor of mankind, let us not lose sight of the pleasing hope that he and his *murderer* shall be remembered as they deserve. Yes! let us cherish the pleasing idea, that while the name of Robespierre shall be remembered with deserved detestation along with those of Nero and Caligula to excite indignation against tyranny and its supporters, that of Lavoisier shall excite in the breast of every votary of science the warmest gratitude!

From the time of Stahl to that of Lavoisier the metals were supposed to be compound bodies, formed by the union of phlogiston with peculiar earthy bases. During their combustion or calcination they were supposed to part with this phlogiston to the surrounding bodies. Even the weight they acquired by this supposed loss of one of their constituent principles did not for a long time shake the belief of the followers of Stahl in their favorite theory. They all seemed eager to discover some opiate by which they might lull their reason to sleep. The celebrated Boyle affirmed that the increase of weight in calcined metals is owing to the combination of the matter of fire. Boerhaave attributed it to the surrounding bodies which deposit themselves upon the metal: While the generality of the followers of the doctrine of phlogiston supposed it to be the principle of *levity.* Such are the powerful effects produced by the union of a great name with any theory whatever; like the head of a monarch stamped upon base metal, it serves to give it currency for a time among the unobserving part of mankind.

At length Lavoisier proved that the increased weight of the calx is owing to the absorption of oxigene, and that

* When the order for his execution was presented to Lavoisier he requested a few days to complete a course of experiments he had commenced, but this was refused, and he was hurried off to the *scaffold.* What may we not have lost!

it is in the exact proportion of the quantity of this gas absorbed. He now undertook to reverse the theory of Becher and Stahl. Instead of supposing that in combustion phlogiston is separated from the combustible body, he accounted for this phænomenon by the body absorbing oxigene from the atmosphere, which he discovered consisted of nearly twenty-eight parts of oxigene united to seventy-two of nitrogene.

The supporters of the doctrine of phlogiston, thinking it in vain to attempt any longer to uphold a system founded on the existence of so chimerical a substance as they had heretofore described, and perceiving that in many cases of the solution of metals in acids inflammable air is generated, declared this hydrogenous gas to be phlogiston in an uncombined state. No sooner had they given to this

——————"airy nothing
"A local habitation and a name,"

than they doomed it to destruction. While it retained its *Protean* powers of at one time being the principle of levity, and at another possessing gravity, it was impossible to grasp it firmly enough to destroy it; but it now became a fair object of discussion.

The French chemists were for some time at a loss to account for this disengagement of hydrogene. At length Mr. Cavendish discovered that water is a compound body, formed by the union of the basis of hydrogene and oxigene. The source from which the inflammable air arises now evidently appeared not to be, as their opponents supposed, from the metal during solution parting with its phlogiston, but from the water combined with the acid being decomposed, its oxigene uniting to the metal whilst its hydrogene is set at liberty.

Lavoisier has applied his theory of the calcination of metals to the phænomena of every other species of com-

bustion with so happy an effect that the doctrine of phlogiston has become almost universally exploded.* That theory, which but a few years since commanded the undissenting voice of the chemical world, is now almost totally forsaken. Still, however, the tottering dome of this once mighty fabric is supported by one solitary pillar, so well constructed as by its single force to uphold it against the warring elements, nor can it ever fall till this pillar is removed. Never can the doctrine of phlogiston be said to be totally destroyed, until it shall cease to rank among its supporters the name of PRIESTLEY.

I shall now present you with the last and most pleasing revolution that has occurred in chemistry. Hitherto we have beheld this science entirely in the hands of *men;* we are now about to behold *women* assert their just, though too long neglected claims, of being participators in the pleasures arising from a knowledge of chemistry. Already have Madame Dacier and Mrs. Macauly established their right to criticism and history. Mrs. Fulhame has now laid such bold claims to chemistry that we can no longer deny the sex the privilege of participating in this science also.† What may we not expect from such an accession of talents? How swiftly will the horizon of knowledge recede before our united labours! And what unbounded pleasure may we not anticipate in treading the paths of science with such companions? ‡

I shall now, gentlemen, conclude with a few observations on the utility of a general diffusion of chemical knowledge throughout America.

Living as we do in a new, extensive, and unexplored

* See note D.

† *Mrs. Fulhame* has lately written an ingenious piece entitled "An Essay on Combustion, with a view to a new art of dyeing and painting, wherein the phlogistic and anti-phlogistic hypotheses are proved erroneous." Since the delivery of this oration she has been elected a corresponding member of this Society.

‡ See note E.

country, separated by an immense ocean from all other civilized nations, we must feel ourselves deeply interested in a knowledge of *its* mineral productions, and this can only be arrived at through the medium of chemistry. As far as our very limited knowledge has yet gone, we have every reason to believe that nature has been far from bestowing her blessing upon it with a parsimonious hand. Abounding as it does with the richest ores of the most valuable metals, we should be committing a crime of the blackest dye, were we through *wilful ignorance* to trample under our feet these invaluable gifts of the CREATOR.

The only true basis on which the INDEPENDENCE of our country can rest are AGRICULTURE and MANUFACTURES. To the promotion of these nothing tends in a higher degree than chemistry. It is this science which teaches man how to correct the bad qualities of the land he cultivates by a proper application of the various species of manure, and it is by means of a knowledge of this science that he is enabled to pursue the metals through the various forms they put on in the earth, separate them from substances which render them useless, and at length manufacture them into the various forms for use and ornament in which we see them. If such are the effects of chemistry, how much should the wish for its promotion be excited in the breast of every American! It is to a general diffusion of a knowledge of this science, next to the VIRTUE of our countrymen, that we are to look for the firm establishment of our INDEPENDENCE. And may your endeavors, GENTLEMEN, in this cause, entitle you to the gratitude of your FELLOW-CITIZENS.

NOTES

NOTE A—p. 21

The origin of alchemy cannot be traced farther back with any certainty than the second or third century of the christian era.

In all probability it owed its birth to the general adoption of the proposition that *"All bodies are but different modifications of the same primitive matter,"* the philosophers supposing that this modification might be changed at pleasure by means of certain chemical agents.

Note B—p. 30

Rey, in the last century, ascribed the increased weight of metallic substances when they are said to have lost their phlogiston, to its true cause, the absorption of air, but on such weak grounds that he is as little entitled to the honour of a discoverer, as a successful dreamer is to that of a prophet; nor can I with justice ascribe this honour to Dr. Hales, though he extracted air from minium; as he imputed the increase of weight not only to the air, but also to sulphur which he imagined is absorbed from air.

Kirwan on Phlogiston.

Note C—p. 31

The almost innumerable technical terms which had been introduced into chemistry, before the formation of the new nomenclature, had for a long time been a cause of general complaint among chemists. The same substance had often eight or ten different names applied to it, most of which either conveyed no idea of its properties, or what is still worse indicated very opposite ones to those it possessed. Within the little time that elapsed from Dr. Black's discovery of carbonic acid it had been known by the names of *Fixed Air, Aerial Acid, Mephitic Acid, Cretaceous Acid,* etc.; but the terms *Oil of Tartar by the Bell, Oil of Vitriol, Butter of Antimony, Butter of Arsenic, Flowers of Zinc,* etc., as applied to these several compositions, are still worse, as they serve not only to burden our memories with a useless quantity of words, but to give us a false idea of the nature of the substances they are put for; as there does not exist in the mineral kingdom, properly speaking, either *Butter, Oil, or Flowers.* A reform in the chemical nomenclature became therefore absolutely necessary to the promotion of science, or rather it became necessary, where so much error existed, to pull down the old system and erect a new one.

In 1782, M. de Morveau proposed a reformation of the nomenclature, and in 1787, M. Lavoisier, by the assistance of many of the best chemists of France, produced the following excellent plan, which is now generally adopted.

1. All those substances which cannot be separated into two or more different principles, by any known process, although they *may* be compound bodies, yet are to be considered, until an analysis can be made, as elementary, and names given to them indicating their principal properties: thus the basis of vital or pure air is called *oxigene* from the Greek words ὀξύς, *acid,* and γείνομαι, *I beget,* as by a union of this substance with certain bases all the acids are formed; and the basis of *inflammable air* is called *hydrogene* from the Greek words ὕδωρ, *water*, and γείνομαι, *I beget,* as it is by a union of this substance with oxigene that water is formed.

2. When two simple substances are united, the name of the compound is to be so formed, by a general rule, as at once to convey the idea of its constituent principles.

Thus all the combinations of those metals with oxigene, which do not by such an union form acids, are called by the general names of *oxides,* as in the case of the union of oxigene with lead forming red lead; which, according to the new nomenclature, is called *oxide of lead.*

According to the new theory the acids are all formed by the union of oxigene with certain bases, the names of the acids are therefore all made by giving to the names of their bases, where they are known, or when their bases are not known to the name of the source from whence they are derived, the general termination *ic.* Thus that acid formed by the union of oxigene and sulphur is called the *sulphuric acid,* and the acid procured from the *Fluor Spar,* the basis of which is unknown, is called *fluoric acid.* But there are acids the bases of which are not fully saturated with oxigene, these are distinguished by the termination *ous,* thus when *sulphur* is not quite saturated with oxigene it is called *sulphureous acid.*

3. The neutral salts are all formed by the union of the different acids with alkaline, earthy or metallic bases. Their names are made by a union of the names of the acids of which they are composed terminating with *at* when they are perfect acids, or

fully saturated with oxigene, and *ite* when they are imperfect, and the names of the bases to which they are united. Thus Glauber's salts, which are formed by the union of the *sulphuric acid* and *soda,* are called *sulphate of soda,* and a combination of the *sulphureous acid* and iron is called *sulphite of iron.*

In favour of this theory of a nomenclature, little need be said, as it bears internal evidence of its utility. Of the immense quantity of technical words which are saved by it I shall give the single instance of the neutral salts.

There are at present thirty acids known, capable of forming neutral salts by their union with three alkalies, eight earths, and fourteen metals, in all twenty-five bases, which would make 750 different neutral salts. If to these we add those which could be formed by many of these acids in a state not fully saturated with oxigene, we shall have not far short of 1,000 different neutral salts. Allowing the former arbitrary mode of naming them to prevail, there can be no doubt that each of these salts on an average would have in the course of time at least two names, we should then have had 2,000 names for them. But happily for the cause of science our memories are saved from being oppressed by this immense mass of technical rubbish by the proper application of the third rule.

For a full account of this nomenclature see the memoirs of Messrs. Lavoisier, De Morveau, Berthollet, De Fourcroy, Hassenfratz, and Adet; first published in the transactions of the Academy of Science in Paris, in 1787, and since translated into English and published by Mr. St. John.

Query. Might not the nomenclature be extended to all combinations of two simple earths by using the name of the earth found in the greatest quantity as a *substantive,* and that of the one found in the least quantity as an adjective. Thus a stone formed by the union of a smaller quantity of *silex* united to a greater quantity of *alumine* would be called a *silicious alumine,* whereas if the *silex* predominated it would be an aluminous silex. It might, perhaps, be also applied to the union of a simple earth with a neutral salt, as in marble, which is composed of *alumine* and *carbonate of lime,* which would then be called *aluminous carbonate of lime?*

NOTE D—p. 35

Lavoisier instead of supposing, with the disciples of Becher and Stahl, that all inflammable bodies possess a certain principle, which they called *phlogiston,* the giving out of which causes all the various phænomena of combustion, says that they entirely arise from the decomposition of *oxigenous gas,* which is a compound body formed by the union of a certain basis with the matter of light and heat,—the basis uniting to the inflammable body while its caloric, or matter of heat, and light are set at liberty. This theory they found upon the following principles.

1. Combustion is never *known* to take place without the presence of oxigene.

2. In every *known* combustion there is an absorption of oxigene.

3. There is an augmentation of weight in the products of combustion equal to the weight of the *oxigene* absorbed.

4. In all combustion there is a disengagement of light and heat.

I shall therefore take the liberty of suggesting the following queries.

Query 1. Should we not consider combustion as an effect of the *elective attraction* between the basis of the gas and the combustible body being stronger than that between the same basis and caloric?

Query 2. If so, would not the same phænomena take place were we to heat a body in any other gas whose basis has a stronger elective attraction to the body than to caloric and light, as do when such bodies are heated in oxigene?

Query 3. In the combustion of hydrogene with oxigene do we not find this to take place? Does not the basis of the hydrogenous gas, which was retained in a gaseous state by its union with caloric and light, unite with the basis of the oxigenous gas, and form water, and at the same time part with its matter of heat and light?

Query 4. Should we conclude because those substances which burn the readiest in oxigene will not burn in any other gas, that no substances are to be found that will? Ought we not on the contrary to seek these substances among those which do not burn

at all, or very slowly in this gas, as the probability is that the same substance, which has a very strong elective attraction to the basis of one gas, will have but a slight one to that of every other?

Respiration may be considered as a slow species of combustion. The oxigene of the atmospheric air inhaled is decomposed, its basis unites to the blood, through the coatings of the blood vessels in the lungs, and gives it a red colour, while its matter of heat is set at liberty and forms the animal heat of the system.

NOTE E—p. 35

The following short extract sets chemistry, as a proper study for females, in so forcible and just a point of view that I cannot refrain from the pleasure of inserting it.

"Chemistry is a science particularly suited to women, suited to their talents and their situation; chemistry is not a science of parade, it affords occupation and infinite variety; it demands no bodily strength, it can be pursued in retirement; it applies immediately to useful and domestic purposes; and whilst the ingenuity of the most inventive mind may be exercised, there is no danger of inflaming the imagination; the judgment is improved, the mind is intent upon realities, the knowledge that is acquired is exact, and the pleasure of the pursuit is a sufficient reward for the labour."

Letters for Literary Ladies.

Some idea of the regard in which the author (T. P. Smith) of the preceding essay was held may be gathered from the following quotation:

While we express our hopes that the whole history of this Columbian mineral will soon be made known, we sincerely deplore the afflicting and untimely death of our friend and countryman, Mr. Thomas P. Smith, from whose industry, acuteness and zeal in chemical (and, indeed, almost the whole circle of physical) researches, Mr. Hatchett

informs the Royal Society he had anticipated important aid in this inquiry.

We think it only a tribute due to justice, on this occasion, to insert the following account of a gentleman, whose memory we cherish with warm affection, and whose fate will be a subject of lasting regret to every friend of science in this country.

City of Washington, October 8, 1802.

Died on the 22nd ultimo, at sea, Thomas P. Smith, in consequence of the bursting of a gun.

Few men merit, and still fewer obtain, a long posthumous fame. Their virtues and talents are generally derived from local or temporary events, with the benefits of which they are forgotten. But the subject of these remarks lived not for himself, the particular spot that gave him birth, or the country of which he was proud to be a citizen. His heart exulted in the happiness, and sympathized in the miseries, of all mankind; while his mind exerted its great energies in their service.

Before he reached the period of manhood, he abandoned the frivolous sports of youth, and applied himself to science. With but feeble advantages of education, at eighteen he was a respectable mathematician, and at twenty an eminent chemist. From this period, nature, in all her forms, attracted his attention, and he incessantly mingled the labours of the closet with an observation penetrating, practical and profound. Shaking off the dull logic and inglorious trammels of the schools, his mind disdained other materials of judgment than well-attested facts, analysed and applied by itself.

Though fascinated to enthusiasm with the charms of natural science, he was not regardless of moral knowledge. He was an early, uniform, inflexible disciple of republican liberty; in his devotion to which he was as firm as the rocks which he so often trod.

Two years ago he went to Europe, principally to extend his qualifications for mineralogical and chemical pursuits. But as the powers of his mind were not circumscribed by common limits, they embraced the whole circle of science. He travelled through England, Germany, France, Sweden, Denmark, and other countries. His associates, wherever he moved, were the learned and the liberal.

With the fruits of wide experience, he sailed, about two months since, for his native land. He had viewed the proudest countries of Europe; but he wrote to his friends that he had seen no country for which he would consent to abandon the United States, where freedom and industry confer happiness. He exulted in the prospect of soon meeting his friends, and in passing in their society, and in literary pursuits, the remainder of his days.

But he has been disappointed. He is gone. With the youth of five and twenty has perished not the blossoms, but the mature fruit of age. Eulogium is often extravagant, but truth sometimes sustains her boldest panegyric: and when she declares that Thomas P. Smith, for science, had no superiors of his age in the United States, and promised, in the progress of life, to have few equals, she pronounces the sacred language of truth.—NATIONAL INTELLIGENCER.

CHAPTER III

THE oration delivered by Smith is a history of the development of chemistry and of chemical theory; it does not contain any new discoveries.

From a number of old manuscripts this additional information concerning the Chemical Society of Philadelphia, its members and purposes, has been gleaned.

Ruschenberger (Institution of College of Physicians of Philadelphia) mentions that "the Chemical Society of Philadelphia held stated meetings weekly, in the Philadelphia Laboratory, or Anatomical Hall. Some of the Fellows of the College of Physicians were members of it. The chief purpose of this association was to acquire information relative to the minerals of the United States. A standing committee of five was charged with the duty of analyzing any mineral which might be submitted to it, provided it were sent free of expense, with an account of the locality and situation in which it was found. The analyses were made without charge. Notice of these terms was published in several newspapers of the United States.

"The officers of the Society in 1802 were James Woodhouse, Pres.; Felix Pascalis and Jno. Redman, Vice-Pres.; Wm. S. Jacobs, Librarian; Wm. Brown, Jno. S. Dorsey, Curates; John Y. Bryant, Treasurer; Thomas Brown, Secretary."

The advertisements of the Society are extremely interesting and worthy of consideration:

1. To the Citizens of the United States.—In consideration of the general utility that would result from the citizens of the United States being able to procure, free from expence, an analysis of any ores or mineral substances, "The Chemical Society of Philadelphia," on the 20th of June, 1797, passed the following resolution:—

"Resolved, That a committee of five members be appointed, whose business it shall be to notify, in the different papers of the United States, and by circular letters, that they will give an analysis of all minerals which may be sent them."

In conformity to the above resolution, we hereby give notice, that we will analyze any mineral which may be sent us, provided it be sent free of expence, and accompanied with an account of the place and situation in which it was found.—

Committee:
Thomas P. Smith, No. 19 N. 5th St.
James Woodhouse, No. 13 Cherry St.
Samuel Cooper, No. 178 S. Front St.
Adam Seybert; No. 191 N. Second St.
John C. Otto, No. 37 N. Fourth St.

The Weekly Magazine, Saturday, Feb. 3d, 1798.

2. To the Citizens of the United States:

The Chemical Society of Philadelphia, desirous of diffusing information throughout the United States relative to the manufacture of Nitre, have appointed a Committee to collect into one view all the different processes carried on for that purpose in different countries.

In pursuance of their appointment, they now take this method of requesting any person who possesses information

relative to the manufacturing of this valuable neutral-salt to forward it to them (post paid).

Information from such persons as carry on manufactories of it with their results would be peculiarly acceptable. They would be obliged to any persons who would furnish them with accurate descriptions of the situation, soil, temperature, &c., &c., in those places in which Nitre is found in a native state.

THOMAS P. SMITH,
19 North Fifth Street
ROBERT PATTERSON,
148 South Fourth Street
JOHN C. OTTO,
37 North Fourth Street
} COMMITTEE.

(*The Medical Repository*, Volume II, page 120 (1799).

3. The Chemical Society of Philadelphia, besides a variety of other minerals from different parts of the United States, have lately received a specimen of the golden auriferous pyrites from Virginia. From 10 pwts. to 13 grains of gold, 24 karats fine, have been extracted.

A quantity of Manganese has been sent to the Society from the County of Albemarle, where it is found in abundance. This mineral now retails in Philadelphia at the rate of 11 pence per pound. It is consumed in this country principally by potters. It is used in Europe in bleaching and in the manufacture of glass. A variety of the sulphate of barytes with lapis hepaticus, accurately described by Cronsted as the liberstein or liverstone of the Germans and Swedes, has also been forwarded to the Society from some place. This mineral almost always accompanies the best metallic ores and is considered by mineralogists as a happy presage of finding them. According to the celebrated Becher, it is a certain indication

aut præsentis aut futuri metalli. It is hoped that the importance of mineral substances in agriculture and manufacturing will induce the farmers and other gentlemen in the United States to attend to the mineral products of their fields and send them to the Chemical Society of Philadelphia, where they will be accurately analyzed, free of expense. By this means many valuable discoveries may be made and we may become acquainted with the operations of nature in this part of the globe.

(*The Medical Repository,* Volume III, page 68, 1800).

4. Among other communications lately made to this association (the Chemical Society of Philadelphia) of the votaries of Science is a series of Geological essays by Mr. George Lee. Though we have not yet heard that the author has given the geology or mineralogy of any part of the United States not hitherto described, yet we learn that, like Mr. Kerwan, he is a spirited supporter of the neptunian theory of the earth and a firm believer in the mosaic account of the deluge and its consequences.

(*The Medical Repository,* Volume IV, page 303, 1801).

5. The Chemical Society of Philadelphia have appropriated $50.00 for the purpose of procuring a medal, which is to be presented to any person who shall produce the best specimen of clay found in the United States and fitted for the manufacture of potter's ware. No attention will be bestowed on clay inferior in quality to that from which the common modern queen's ware is manufactured or on that which shall have been found in such a situation and quantity as that it may be obtained and manufactured with convenience and profit. Any person, who shall be acquainted with clay, an exhibition of which may entitle him to be a candidate for the possession of the medal, is requested to deliver a specimen of such clay to one of the corresponding secretaries of the Society before the first

day of January, 1804. Together with any specimens of clay sufficient evidence of its good qualities and an account of the place and quantity in which it may be found must be delivered and every communication on this subject must be accompanied by a sealed note, containing the name and residence of the author. The medal will be adjudicated soon after the day above mentioned. The corresponding secretaries of the Chemical Society for the present year are Dr. John C. Otto, Mr. John Y. Bryant and the undersigned.

By order of the Society,

R. HARE, JR.

Philadelphia, February 4th, 1802.

(*The Medical Repository,* Volume V, page 349, 1802).

A second annual address before this pioneer society illustrates the emphasis laid by its membership upon the importance of chemical discovery:

ANNUAL ORATION

DELIVERED BEFORE THE

CHEMICAL SOCIETY OF PHILADELPHIA,

January 31st, 1801.

BY FELIX PASCALIS, M.D.

VICE PRESIDENT OF THE SOCIETY

Published by Order of the Society.

Nous avons l'avantage de voir enfin,
les plus beaux jours de la Chimie.—
MACQUER.

PHILADELPHIA:

Printed by John Bioren, No. 88,
Chestnut Street

1802.

ANNUAL ORATION, ETC.

Gentlemen of the Chemical Society,

I come, this day, to fulfil the honourable task of addressing your society on subjects relative to Chemistry.

Permit me, at first, to congratulate you on the return of another year to be added to your commendable exertions for the improvement of a science which constitutes true philosophy, and imparts so many advantages to enlightened and polite nations.

Of these none have remained unknown to you, for, as soon as this institution was formed, by the talents and diligence of its members, it stood adequate to the experimental researches pursued by the celebrated schools of Europe. Less censurable than those venerable seminaries of learning which have spent so many years in false theories, in idle and useless systems, you have had the satisfaction of participating with them in their discoveries as early as your members could controvert, with the learned of all the world, any of the subjects or causes of the revolution that that science has experienced, within these few years. By whatever consideration personal praise could be waived, in your assembled Body, no doubt, it remains deserved by those who early promoted the sedulous cultivation of the most useful science. I subscribed to this laudable intention when I had the honour of being called as a member in your institution. But I scarcely can confide in myself this day, when, by your appointment, I am to display some of the admirable laws and numerous advantages of Chemistry. Was I adequate to the task I would publicly declare that I became so among you, and after many years of our scientifical intercourse.

Necessity was the parent of our science in the most

distant ages that historical records can trace; from avarice and cupidity it afterwards received some slow and obscure improvements, but accurate analysis only has lately brought it to perfection. The ancient history of Chemistry offers such a lamentable view of ignorance, superstition, and empiricism that its pages seem no more useful but to prove how laborious, slow, and uncertain is the advancement of human understanding, unless it is aided by the correct results of observation and by an unprejudiced love of truth. Happy is our age, in which, at last, we are acquainted with the elementary laws of existing bodies! Those laws which extend to all material objects, visible or invisible, known or still concealed from our observation; those laws, the limits of which we do not know, because we cannot trace where the limits of nature are to be marked; those laws form and constitute the science of Chemistry; indeed, by its principles it is connected with all the branches of natural philosophy, and by its comparative results it dictates the rules of arts and the processes of manufactories. Under this twofold view Chemistry, embracing all wants and comforts of mankind, is now to be contemplated as the most important and interesting subject, altogether to do honour to your pursuits in its study, and to encourage many more votaries to the acquisition of its numerous advantages.

Philosophy. You know, Gentlemen, how numerous are the branches of universal Philosophy! They form like a beautiful tree, which often has been drawn by the hand of genius; under its shade all sages vied to find shelter or repose, and of its fruits they all wished to partake. When all these branches have been severally examined and studied they appear so well connected, and so much depending on the same laws, that they compose but one science, the mysteries of which cannot be disclosed, unless we enter into the laboratory of the chemist, and there we explore its processes. From these, all sciences in the

physical order must receive their tenets and elementary doctrines.

Astronomy. Would the Astronomer refuse to witness our invariable results of attraction and gravity because we cannot explain their operations, as he does for the rolling sun and planets by the *direct ratio of masses* and by the *inverse ratio of the square of distances:* we must then propose some difficulties arising from the known characters of Caloric. That this element is not ponderous and has no gravity to any given center of density, that no law but that of a projectile power can be attributed to it, is beyond doubt. How is it, then, that all the bodies of the planetary system, the greatest number of which are solar or ignited, could equally obey the same law of the direct ratio of masses and of the inverse ratio of the square of distances? Newton had calculated that the comet of the 8th of December, 1680, when in its perihelium had received a heat 2,000 times greater than that of red hot iron. What matter, we ask, can be conceived to exist, as a center of gravity, at a still more and infinitely higher degree of heat? The known laws of nature leave no room to any conjecture, except to that of an elementary and homogeneous fire, which cannot exert but an immense projectile power, that, of course, it excludes entirely any share of attraction by the direct ratio of masses and the inverse ratio of the square ratio of distances. A conclusion more than probable is therefore to be drawn that to chemistry is, perhaps, reserved to disclose a different order of the primary laws of the universe.

Caloric. In this, as well as in other investigations, the philosopher must be aided by the results of experimental chemistry. Any proportion, for instance, of the attributes of matter—is it well understood, unless the laws of Caloric are correctly defined? Caloric! astonishing principle of destruction and life! To describe well its activity and operations it would be necessary to advert to mountains

which it undermines, to the frightful craters it opens on their most elevated regions, and to the immense torrents of lava with which it inundates afterwards cities and empires. We might thence follow its burning streams into the abyss from whence it again breaks open its barriers, lifting mountains, raising islands, or, if at liberty, uniting to water, boiling up in the shape of whirlwinds or clouds which darken the firmament. This indestructible agent then divides itself in various scintillating meteors, or by its sudden combination with air, forms the lightning, and by tremendous electrical detonations, spreads terror and devastation among mankind, threatens nature with the perturbation of all established order. However, you may master Caloric, Gentlemen, you may attract it from the regions above under your lens and concentrate it in your crucibles, where, to your command, it will melt or volatilize the hardest metals, reduce rocks to the elasticity of clay, and clay itself to the adamantine hardness; where it will be disposed by your processes into fixed and opaque substances to arrange their pores for an easy passage of the light! With you it will create, as it were, aerial and invisible bodies among all the known substances you can enumerate and those that can be suspended in a gaseous state. After such comparative results philosophers may explain elementary laws concerning the *attributes of matter;* they are able to explain, likewise, the revolutions of seasons and the phenomena of the atmosphere. With the capillary tubes of observation they may measure spaces, density of air, and gravity of fluids. How often scholastic philosophy blundered on those simple subjects when chemistry had not yet controlled all other sciences! The first who measured the parabola of a bullet launched into the air by the thundering explosion of a cannon attributed the almost incandescent heat of the metallic globe to the detonating mixture, because to chemistry only it belonged to demonstrate the power of friction in disengaging heat

from its latent recesses. Other erroneous doctrines on that noble agent of nature which so often has been mistaken for a modification or produce of matter have been exploded! The supreme laws of the existence and diffusion of Caloric in nature are its necessary tendency to equilibrium with external temperature, and its power of overcoming the cohesion of particles to satisfy its affinity with them. But let us not pass over that other phenomenon of affinity unnoticed. Without the knowledge of its laws philosophy would be mute at the view of the stupendous works of nature, or she would be obliged to conceive as many deities as there are prodigies in the creation.

Affinity. This is the great power which arranges, unites, and hardens homogeneous particles of matter and exerts itself likewise with due proportion upon heterogeneous substances. Under the heavy foundation of mountains, in the deepest subterraneous cavities of the earth, in the bottom of the seas, no where it finds obstacles sufficient to oppose its operation, except Caloric accumulated could suspend their effects to a certain degree. Yet distance can truly impair or weaken the power of affinity; approximation, of course, will augment it. In itself it is composed of many tendencies, but these remain incommensurable. Their insulated effect is nil, and, as soon as they are simultaneous, they become effectual. In fine, affinity, which is attraction, adhesion, cohesion, and aggregation, by the immediate result of elective power is the primary cause of dissolution and decomposition. Now let the philosopher listen to the laws of affinity, and the minutest circumstances will be explained, from the spheric form of a bubble of air, or of a drop of water, to the various degrees of elevation of fluid in capillary tubes, or to their pressure by the same, both effected in inverse ratio of the squares of diameters. To these we may add the phenomenon of blaze in ignited bodies, the mechanism of breathing and renewing animal heat by the decomposition of common air, but,

more especially, the effects of the refraction of light. The philosopher may, no doubt, well explain how the density of the rays of light is like the square of the distance from its focus; how it is transmitted to the retina; in what angle it is reflected and at last refracted. To these elementary laws of optics—catoptricks and dioptricks—the chemist adds a question, ready to answer it: What is light?

Light. Light, which adorns the whole creation by an infinite variety of colours, because each colour is light presented in a different angle of refraction or refrangibility; light is the element of all the worlds, of innumerable solar systems and comets; light and caloric are as often united as vital air and Azote are aggregated to form the atmospheric orbit of the earth; yet, caloric may be present without light, and vice versa: light ranks the first among the elements of nature by its tenuity and elasticity, for, more rapidly than any other, it can traverse the immensity of spaces. It cannot be said to be like a modification of a diffused subtile matter, because we trace substances, with which it has an elective affinity; it stimulates animated bodies, and to vegetables, it is as a last component part necessary to develope their succulent juices, their splendid flowers, their robust fibres, and their luxuriant colours. No paradoxical doctrine will ever be able to erase from the books of Philosophy such truths inscribed in it, by the hand of the chemist.

Atmosphere. Behold! another field is open; but to the most ingenuous philosopher it will appear a dreary solitude, an immense desert, until Chemistry discovers in it, all the elements of the productions of nature. Air, that invisible fluid, but so sensibly perceived by our organs, has long been the object of innumerable investigations, yet in our days only, they have been successful. The learned among the ancients, filled up their atmosphere with their Genii, to explain contrary effects of that invisible orbit; and among moderns, it has long been a de-

sideratum to account for an element which appeared necessary to fertilize the fields, and to nourish life, while it was dreaded as the destroyer of agricultural labour, or the vehicle of contagion and death among mankind. The attributes of its particles were still more incomprehensible, when it was considered that with a rapidity to be computed only by the imagination it could transmit light and sound, although they would be agitated in a thousand contrary directions. The pressure which it exercises round the globe, to the center of which it gravitates, was, at first, discovered by Torricelly, who traced to what height a column of fluid could be equal by its weight to the superior pressing column. More of its comparative gravity with other fluids was still better defined, when Monge declared that, if the whole atmosphere could disappear, all the liquids on earth would suddenly rise, be converted into vapour, and form another atmosphere; but all these progressive views scarcely unveiled one corner of a more extended view of the laws of nature. The chemist attempted the analysis and synthesis of atmospheric air, and it was performed; moreover, he found out, and demonstrated, that its component parts could be equally concrete and fixed in organic bodies, or combined with other various solid substances; it has then been proclaimed that the Oxygen and Azote constituting common air were nothing but another modification of matter itself, and a continuation of the chain of existing bodies. It was no more problematic afterwards, to ascertain how this fluid administered life to us, as often as there are instants during which we are allowed to exist, or how it effected the most principal changes and modifications in the animated or inanimated creation. The pneumatic observer could soon disengage the component parts of atmospheric air, from common materials, and imitate a new creation. Let honour be given to the memory of the great Newton; he was the first who predicted the prodigies of our laboratories: "Si

se tangerent," said he, "particulæ aeris—aër evaderet in marmor,"* but without the science of Chemistry, that oracle could never be explained, and philosophy could not advocate as a true proposition, that the atmosphere is the last reservoir where the elements of all bodies are ultimately received, and from where again they are subtracted to create bodies and to support organic life: there is demonstrated another axiom long ago proclaimed by Lucretius, *"nothing is annihilated in nature."* As soon as she has effected a dissolution, she exhibits in her very bosom the consoling power of a new creation!

But from the elevated regions of Heaven, and from all the primary elements and phenomena of the world, the mysteries of which have been disclosed by Chemistry, let us more minutely fix our attention on the surface of our globe. A more admirable view of the analysis and combinations of all existing bodies; their formation, growth, alteration and destruction, their treasures and deleterious qualities, every thing will be unveiled by our processes. More evidently we will then enumerate the infinite improvements which are applied to arts and manufacture by mineral, animal, and vegetable Chemistry.

Mineralogy. You are fully acquainted, gentlemen, with the difficulties and confusion which attended the science of mineralogy, when laborious researches and observations were transmitted through external and erroneous characters. Some such as Gellert and Wallerius could not distinguish any thing else but vitrescible matters, or argillaceous or apyres, or alkalines and calcarious: others would prefer the general division of earths, sands, and stones. Some simply divided minerals into earths, salts, combustible and metallic substances. As late as the year 1784, the famous Daubenton took another erroneous classification, consisting of negative characters, of insoluble, incombustible, metallic, then transparent, and crystallized, smooth,

* Tract. de nat. acid.

&c. Now we may observe, that these descriptive methods could not add the least improvement to science, and that they contributed to many erroneous assertions. It is not, for instance, by the presence of a metal that the ponderous spar is to be judged, since its basis is a primitive earth, and if this is a Tungstat of lime, or if the Wolfram itself (which is thought, by many, another kind of primitive earth) is nothing but an ore of Tungstein, we are to be determined in these various opinions by accurate analysis only, and in no ways by external characters. Were these to fix our mineralogical definitions, by what given habitudes would we be justified to class the beautiful fluoric spars of Derbyshire, or fluat of lime, among insoluble neutral salts? Error would be still more egregious and unavoidable between the Sulphures or Molybdena and the Carbures of iron, which are so absolutely resembling each other. It has been found that a Borate of lime may spark and scintillate as well as quartzose substances; that metallic carbonates must effervesce with acids, in short, there is not one descriptive method, but it will in many points lead us to contradictory facts. So sensibly was Daubenton convinced of their deficiencies, that he expressed his wishes for a method founded on analysis of constituent principles. Fourcroy begun it in 1780, but the Scyagraphy of Bergman still better demonstrated the superiority of the method of constituent principles. This doctrine was embraced by Monge, who retained external character for varieties only; by Kirwan of England, Werner, Dezbern, Cronstedt and Chaptal; thus the science of Mineralogy was forever added to the dominion of Chemistry.

With this conquest, we will not however consider it as the chief point in Chemistry, to form theories on the primitive state of the earth, to explore the ruins of extinguished volcanoes, nor the most antique works of nature, from the grotto of Fingal to that of Antiparos. We will

not indulge to the idle curiosity of enumerating the causes of the granitic ridges of mountains and of the calcareous secondary ones; we may dispense with contemplating whether the ocean has once been an atmospheric orbit, during the original conflagration of the Earth, or how, after its condensation it insensibly retired from the polary regions, and elevated plains of Tartary, to its Pacific and Atlantic bed, leaving everywhere immense masses of animal and vegetable productions. However useful to science, these mineralogical essays and deductions would be, with more real advantage, the chemist will be contented to analyze mineral substances, and to procure those which satisfy our wants, constitute our wealth, augment our comforts, which enriches our arts, and manufactures.

Nitre. Among these, Nitre may justly occupy the first rank. Any thing relative to the production of that precious saline matter is interesting as much to save expensive importations, as to perfect manufacturing processes. Let me remind you of that awful period of the French revolution, when the fate of 30 millions of people, divided, confused and famished, ruined and surrounded by sea and land, seemed to depend on a sudden formation of saltpetre, that only means of defence, which was exhausted. Various ingenious ways were devised by the learned,—saltpetre was formed,—and the tyrant's phalanx being dispersed, Liberty was triumphantly obtained, thanks to analytic science. To all the known processes which may procure Nitre either from artificial beds, or lixiviation of certain substances, we now may add that of the decomposition of common metallic oxyds, and of ammoniac; but the most surprising, if further experiments could evince its efficacy, would be that of procuring the formation of nitric acid by an only tenfold *compressibility* of the aggregate Oxygen and Azote composing atmospheric air. Various attempts with ingenious apparatuses promise great success, and if Citizen Guyton is not disappointed in his expectation, with grati-

tude, Chemistry will receive from him the power of accelerating the operations of Nature.

Oxygenated Muriat of Potash. It was during that lamentable period when the friends of Liberty were, with importunity, compelling nature to supply them with some new means of defence against their foes that the discovery of Berthollet, on the tremendous fulmination of Oxygenated Muriat of Potash was again resumed.—In this age of reason and benevolence, let us never boast of increasing the power of destruction, since the prevailing Philanthropy exerts itself against any system of warfare. We only remark, therefore, that from various habitudes of the *Hyper*-oxygenated Muriat of Potash, the most surprising effects are to be reckoned among the laws of nature. If you take about 20 Centigrammes, or 3 grs. of that saline matter, with one third of pulverized Sulphur, you may by a slight trituration, produce several detached detonations: but wrap up the mixture with paper, put it between an anvil and a hammer, strike a blow and the detonation will be equal to that of a cannon, surrounding you with a purple blaze, and white smoke.—With several other mixtures of that salt, shock or percussion will equally operate with the most tremendous power. This mechanical effect is therefore equal to that of caloric, or of fire communicated from one body to another. This singular phenomenon had been witnessed, although in a very small degree, in the gun powder, but it had not been attended, in this point of violence, that, by compression, the particles of Oxygen could unite to those of inflammable bodies, and form an abundant gazeous fluid to which a great quantity of caloric, gives such electricity as to strike on the air with an unparalleled violence.

Under the unremitting exertions of chemists, Mineralogical enquiries have developed as many curious facts as there were improvements to be added to all known processes, for obtaining muriatic acid, for cheaply crystallizing

the sea salt, for extracting the soda, for preparing the muriat of ammoniac, for purifying all combustible minerals that can be converted to our use, for preparing metals, and converting them to the most useful purposes, for adding new ones to the treasures we were already possessed of; but one of the discoveries of the latter kind is too remarkable not to be fully detailed here.

Cast Steel. Long before the Phlogiston of Stahl had been exploded by the new analysis, and in spite of all the beautiful experiments of Bergman and Priestley, nothing very certain was known on the fabrication of various sorts of steel. Not longer than twelve years ago, it was discovered at last that carbone only, in due proportion, would constitute various sorts of fused iron, and of steel: But the English, who had been long in possession of a more perfect kind of cast steel, could not in the least be disturbed by the repeated researches of Vandermonde, Monge and Berthollet. The latter had ever declared that he was at a loss to account for the character of that precious metal. But a mystery which had been perhaps fortuitously discovered, which avarice and cupidity had so long concealed, was at last disclosed by the ingenious Citizen Clouet and made public with a liberality which altogether honours science and the National character. The whole secret was I believe, found out by analogy; as different quantities of fused iron and steel were known to be the result of various proportions of Carbone and of few vitrescible substances of the original ore. It was therefore concluded that a more intimate union of that element, and of the purest clay, with the best iron, effected spontaneously at a due degree of heat, could form the famous English cast steel. Liberal private means, and even national expenditures have been largely and sedulously applied to numerous experiments, at the wind furnace of Macquer, to the 150th degree of Wedgwood's Pyrometer. The results, gentlemen, have magnificently equaled the

most admired steel of Huntsman and Marshall. The new theory of our chemistry has besides acquired another illustration of the elective affinities of iron, for Carbone, since the Carbonate of lime, or marble in dust has been offered to it. The fixed air has been decomposed, and the results have been equally successful. So much for the wonderful secret of the cast steel of Sheffield, Yorkshire.

Alumine. Before I terminate this article of mineral chemistry, let me remind you, gentlemen, of the great advantages obtained by other nations, from the class of alumines only. Among the compounds in which that primitive earth is predominating, they have found all the materials from the common tile and brick to the most elegant works of Porcelain. Not only various kinds of clay have rendered infinite services for the commodities of life, but they may be turned into the best instruments of arts and utensils of manufactures of glass. Some clays have been found likewise to be good manures, and others are an excellent substitute for soap in the Fuller's art.

That America possesses such treasures, it is needless to prove, and that they have not yet been applied to the use of the community, it is also an object of regret. Do, gentlemen, let a liberal patriotism animate your scientifical pursuits, among you who are to see the glorious days of the trans-Atlantic Republic. Encourage and repeat mineralogical experiments on all kinds of Alumine, the first who will successfully procure manufactured works of the kind and tolerably good earthen wares will deserve well of his country and be rewarded by the gifts of fortune.

Vegetable Chemistry. By offering you a few more observations on Vegetable chemistry, I hope I will further illustrate the dominion of that science over all the branches of natural Philosophy. Indeed, not only the most interesting materials upon which we operate must be obtained from vegetables through the processes of combination,

but the chemist moreover is called upon to explain the phenomena of vegetation.

Vegetable analysis has offered us three elements only, in vegetation: Carbone, Hydrogen, and Oxygen. The number and proportion of these principles will afterwards be sufficient to answer any question on different other results of vegetation.

Observation demonstrates that water and Carbone are the real nutritive principles of vegetation, that is to say: Hydrogen and Oxygen are afforded to them by the decomposition of water, while Carbone is procured from the decomposition of animal and vegetable matter. All this is confirmed by the analysis of the fibrous part of plants which are a mere aggregate of Carbone. But in what manner is Carbone carried into all the parts of a plant or tree? By what means can it circulate in them? by what solvent is it rendered the precious food of vegetables? The solution of all these problems, gentlemen, is provided by correct and incontrovertible experiments.

Pure charcoal such as it is left in our hearths, or mixed with any kind of pure and dry earth, could not certainly be spread on the ground, and depended on as the best manure. But on the other hand, do not we know that dead vegetables (from which much carbone can otherwise be procured by combustion), when relaxed or softened by maceration and putrefaction, are indeed the best materials for an excellent manure? How shall we account then, for the striking difference of pure carbone being ineffectual for vegetation, and of compounds of carbone becoming so evidently necessary to it? why, in the latter case, that element is truly held in solution, by oily, extractive, alkaline and resinous vehicles. Now water, which has the faculty to dilute those natural combinations, becomes itself the solvent which carries carbone through all the system of vegetation, and by which nutrition and digestion are accomplished.

To proceed to the last stage of formation and growth of vegetables, let us mention, that air, and perhaps azote, caloric, acids, motion, and even rest, may suffice to precipitate the carbone. These other eventual agents therefore, which everywhere are found active, are those that effect the concretion and growth of any fibrous matter—these are wholly explaining the principles and mysteries of life in vegetation, because they support and animate its organs, distribute the nutritive matter, modify the action of perturbating causes, preside over all the operations of that living laboratory of nature, just as the Chemist directs the operations of his own, and changes the results by altering the form or number of his reagents.

These general principles being established by analysis and synthesis, who can deny that Chemistry teaches the natural philosopher, how in the system of Nature, primary substances can be arranged so as to form organized bodies, . . . and from thence, obtain such attributes with which we were formerly unacquainted, active attributes! which we never dare assign to matter! But if nutrition, growth, and decomposition of plants, are explained by the laws of chemistry, how much better any precepts respecting the choice of their soil, climate and temperature, of their management and multiplication, will be derived from the same science? not only agricultural rules are connected with Chemistry, but it dictates likewise a kind of vegetable Medicine, which has its institute in Agriculture, of Hygiene, of Clinics and of Theurapeutics.

Vegetables as objects of analysis are to be considered as containing substances and compounds necessary to our pleasures and comforts, to arts and manufactures. Such are mucilages, oils, rosins, fecula, gluten, Sugar, various acids, colouring matter, wood, extractive substance, the Aroma, and the Oxygenous gas which all vegetables emit by their excretory organs; of these subjects I can now notice but few.

Nutritive Principles in Vegetation. It was very difficult formerly to define which of the component parts of vegetables would afford a wholesome and nutritive food. The mucilage which exists in them, variously elaborated with acids, gums or sugar, has been at last pointed out. Analysis has even traced that food in the abundant and pulverulent fecula which constitutes the greatest quantity of substance in our grains, roots and seeds, the same principle admirably combined or deposited in different organs of plants is the very means of their propagation, growth, and fructification. In whatever plant it is more or less abundant, in what parts it can be more or less concentrated, it cannot escape the analysing investigation of the chemist who might extract a wholesome food from rejected vegetables and insipid roots.

Other treasures, gentlemen, are discovered by the leading method of analysis. As soon as oil, that precious vegetable produce, was known to be an intimate compound of Carbone and Hydrogen, as soon as its various gelatinous states, fixed and volatile, were ascertained the very organs of the plants in which it is abundant, were easily detected. Hence by comparative results of one known species bearing resemblance to another, by the shape, taste, analysis, of their pulps and seeds, it was found out that common bushes spontaneously growing, might supply us with one of the most indispensably necessary products, at least for lamps and manufactures. Such are the *Onopordum Acanthium,* and the *Evonimus Europœus.*

The union of these oils, with alkalies to form soaps is the only advantage that had been obtained, but that they can also, through various processes, combine with earths, acids and oxyds, it has been minutely described, compared and experimented. Pharmacy has therefore already appropriated those processes to several preparations more useful and agreeable.—Poisonous and metallic oxyds the only trituration of which was highly dangerous, have been

safely elaborated while enchained by oil, which has been afterwards easily removed; the art of painting has likewise been enriched with more imperishable colours. Already Citizen Merime, with oil and Copper has splendidly ornamented apartments with a new and inimitable *Verd Antique.*

Acetite and Acetate of Copper. Acetite of Lead. On the other hand, it was well known that certain vegetable acids when united to metallic oxyds, could procure precious materials for painting and dyeing. But the simplest and cheapest processes were never reduced to easy principles that could correspond with mechanical means and with an useful routine among poor manufactories: if such an improvement has been obtained from chemistry, it must be confessed, that it imparts its treasures even to the industry of the ignorant, and does not permit that they should be monopolized by a few individuals.—It had been a branch of commerce and considerable revenue, anciently established among the people of large districts of France, to prepare the Acetite and the Acetate of copper, with the acetite of lead. They could not certainly make a more profitable use of the remaining grounds of their grapes, after the vinous fermentation. Their mechanical and easy operations were always the same, handed down without improvement from fathers to children, until they were threatened to lose their annual produce by the concurrence of the Hollanders who manufactured and introduced, much purer and finer Acetite and of course preferred, chiefly for paints. The chemists of France immediately inquired into the causes of the inferiority of the metallic oxyds; they found it put in the great quantity of extractive principles in the grounds of grapes, and in several operations of the processes which were mechanically executed; they devised everything accordingly; they traced to the poorest and most ignorant, the way of simplifying their work; they substituted distilled vinegar in some

cases, so that in a little time the most beautiful oxyds and crystals were obtained; the danger of any concurrence was removed among thousands and thousands, of their grateful countrymen.

What improvements were by the same time added to the tanneries of the country, under the philanthropic and ingenious exertions of Citizen Seguin! He accurately demonstrated the theory of the singular effects of the tanning principle which was formerly quite undefined.

This is the Gallic acid which dissolves the gelatinous matter, and precipitates it to consolidation, just by its various degrees of concentration. From the discovery it was concluded that the art of dyeing was in many respects a colouring tannage which should not only please the eye by flattering colours, but should remove even the solubility, or the corruptible tendency of woolen cloths. This advantage has been already experienced in the hospitals and armies. If the tanning principle is too abundant in colours extracted from wood so as to oppose their beauty, it is vice-versa concentrated by gelatinous matter, in short the quality of all coloured silks, wools and cottons, depends entirely on the colouring matters, and on its mordent. I would not, gentlemen, fatigue your attention by a long series of other precious facts, every day obtained from Chemistry in objects of the greatest utility. Let it be said only that they are all admirable and ingenious. Such is the reduction of mordents to as few ingredients as possible, for the preservation of Cloths, just by previous impregnation of oil; such has been that of reddening by acids, the blue colouring matter of the Brazilian Wood; that of dividing two colours confused in one, and the beautiful Yellow and Red in the *Carthamus Tinctorious;* that of employing fermentation to destroy the extractive principle that contaminates the colours; that in every case, of counteracting the development of the red by the Alkalis, and the incomparable process, in fine, introduced

in manufactories of Cloth, by Chaptal, to supply the Soap necessary to mill, to cleanse, and to felt, at the proportion of 48 pounds for 100 of cloths, with an animal soap obtained by the decomposition and solution in Alkaline lixivium, of all woollen rags, and worn out materials that are rejected in various operations in manufactures of that kind. It is necessary to add that no inconveniency in this economical process has been discovered which has not been effectually removed. These and other facts are very simple, but they require the penetrating attention of the Chemist, to be referred to the fundamental elements of the science, ever before they are applied to uses, experiments, rules of art, and to processes of manufactures.

Animal Chemistry. Passing through a great number of discoveries and useful results, for which we are indebted to vegetable chemistry, I come to the last part of our division, that of animal Chemistry.

Medicine. With that definition, you anticipate me in the series of late discoveries and extensive improvements which in all its branches, Medicine has received from Chemistry. That science which directs all its pursuits to the preservation of health and life, to the relief and cure of human diseases, had been reduced during many ages (and by the most unaccountable fatality) to absurd systems, or had been composed in its institutes, of few good precepts, and so few accurate observations that it was inadequate to the wants, or little entitled to the confidence of mankind. About the beginning of the last century, however, the institutes of Medicine appeared to be connected with as many of the physical laws, as can be applied to Physiology. Yet various contradictory systems continued to be advocated, without much retarding the progress of that science. But alas! in a period not much remote from our days, it has been again confused with metaphysical entities that have no connection at all with the laws of nature, and which as attributes of animated

matter are unintelligible, erroneous and absurd. At last, in imitation of chemistry, the spirit of analysis has prevailed in all the branches of natural philosophy, and consequently the friends of the healing art, who wished independently and usefully to pursue the career of their labour, have renounced all logical systems, and composed their institutes of medicine of such facts and aphorisms of the ancient and modern, that experience had rendered incontrovertible with all the results that physical laws and analytic animal chemistry, could consistently offer to their medical investigation.

But here, Gentlemen, let me bear evidence against the unrestrained spirit of novelty and the unphilosophical theories which of late have been mistaken for physiological and Chemico-medical improvements. For whatever is handed to us of the results of animal chemistry useful to Medicine, is sufficient to recommend the connection of both sciences, without any gratuitous, singular and perhaps false applications. I do not pretend to controvert here the supposed discoveries of Girtanner, of Beddoes, Davis and others, who with so rapid strides through the scabrous path of science, have promised to themselves and the public, to open a new Era, and to dispel from among mankind all the diseases they were necessarily subject to. There are, Gentlemen, enthusiasts and fanatics among Philosophers, as well as among devotees and sectaries. Human understanding and science may likewise be degraded by assumed notions and fanciful theories, as much as they can be by the effect of ignorance. Indeed those Philosophers of Germany who pretended once to explain the case of a child born with a golden tooth, without having previously ascertained the evidence of the fact, were no less ridiculous than new theorists deserve to be thought, when they pretend to explain animal irritability by Oxygenation, and to cure diseases, with certain gases, without having in the least established how much and in what respect the at-

tributes of animated matter, could be assimilated to any of the laws of elective affinity, nor to the habitudes of the elementary principles in nature. No, no, the Loco-motive power of life, or rather, animal irritability has nothing in its effects that can be connected to those of the substances that we can torture by chemical processes. The simple contractibility of a muscular fibre, or the active secretion effected in the cavity of a viscus, will ever baffle our enquiries, and all the vast comparative systems, which we would derive the most ingeniously, from the laws, habitudes and combinations of the existing inanimated bodies.

Physiology. But if theories on organized and animated matter, or physiological causes of life and health, and on the origin of diseases, are to form a book of aphorisms perfectly distinct and different from the elements of Chemistry, there are however, certain points of contact, between the functions of our organs, and all the laws of nature, in which, much good has been done; important discoveries have been made and many more are to be expected in medical science, from Analysis and animal Chemistry. To it, for instance, we are indebted for the knowledge of that kind of combustion of atmospheric air, which is effected in the lungs, of the Oxygenation of the blood, of the origin of animal heat, and of interesting conjectures, supported by experimental observations concerning the causes and characters of malignant fevers, and of their inflammatory or anomalous symptoms. To these great acquisitions we must add the Analysis of animal solids and fluids, the component parts of which have been admirably enumerated, and likewise some mysteries of their growth, distribution and final dissolution.

Pathology. Of course, new Pathological views have been judiciously offered, respecting certain visible or invisible agents, which cause perturbation of animal life; among them, deleterious gases, by their operation on animal irritability, stimulant, sedative or poisonous, have

really disclosed a long series of our diseases. Animal Acids, chiefly, and other primary combinations in the blood, in the bile, in the bones, in earthy concretions and others do form, Gentlemen, the most precious collection of facts and observations, that ever medical science could be improved with, for the relief and cure of a good many diseases. Let me mention one only, to prove the useful applications of chemistry to the science of medicine.

Phosphorus. Phosphoric acid, obtained from human bones, and of that produced by the deflagration of Phosphorus, lately induced Vaucquelin and Fourcroy, minutely to investigate the habitudes of the Animal substance. Their experiments accurately pursued in various Analytic and Synthetic ways, proved beyond any doubt, that when 100 parts of Phosphate of Lime, were treated by mineral acids, any of these however, concentrated could not precipitate but 24 parts of Lime, and that of the 76 remaining, 17 only escaped as pure Phosphoric acid, while 59 parts of acidulated Phosphate of Lime were kept, in solution in the Sulphuric, or Nitric, or Muriatic acid. But these 59 parts being again decomposed by the Oxalic acid, or more cheaply by the Nitrate or Acetite of Lead, would afford the whole proportion of Phosphoric acid, which with the above portion of 17, effected exactly 41 parts of that acid, which can saturate 59 of Lime to form 100 parts of Phosphate of Lime. Without mentioning here the reasons they assigned for that singular order of combinations nor the incontrovertible facts by which it is demonstrated, I come to the conclusion. Phosphoric acid has then the power of holding in solution the Phosphate of Lime, and strange to say, in that state is but partly attacked by mineral acids, while it entirely yields to the power of vegetable acids. Therefore, Phosphoric acid, must be powerful enough to soften, decay and distort the bones; and to its superabundant presence only, such disorders in those solids must be ascribed. Moreover, if by any cause whatever,

the usual secretion of Phosphoric acid effected through urine, is interrupted, the consequences will necessarily be injurious to the very support of our frames, by attenuating the Phosphate of lime of the bones, by causing it to deviate in our fluids, or in membranous parts, where it will accumulate, or obstruct and torture the most delicate organs. Now, such is, in this instance, the help we have received from Chemistry, that we may oppose to the greatest ravages in animal economy, if we know which is the superabundant principle that must be counteracted: and thus, in an infinite number of other cases, Chemistry teaches the Physician, on what necessary combinations animal functions are depending, by what assemblage of substance, Providence has marked the order of succeeding periods of Life.—I had almost said, even of Death; because, when that other modification of existence takes place—Chemistry can still more triumphantly operate on that remnant of a Divine work, disunite all its aggregate compounds, and trace their particles to their original elements!

Pharmacy. This is not all; it is interesting to consider a moment with what simplicity and regularity, Chemistry has in general, composed our precepts of Theurapeuticks and methods of Pharmacy. How long and how often the exhibition of remedies has been confused by Empiricisms, embarrassed by ignorance and endangered by avarice! The learned of all ages ever lamented the almost inseparable evils of that branch of the healing art, but their regrets or their cares were ever inadequate to the accurate knowledge of the virtual properties of remedies, and of their proper classification. Behold! now Chemistry has swept off all the dregs of quackery and ignorance; it has detected the imposition of useless compounds, the fallacy of celebrated nostrums, and has exposed the danger, or the inutility of wondrous specifics which had been handed down by a credulous or fanatic care, as the most powerful agents in the cure of diseases. The Revolution in

Science, as effectual as that in the political order, has equally silenced every kind of assuming authority, and of fanatic delusion. The universality of Analysis among all the productions and bodies in nature, has traced, all laws, all virtual properties, and almost all possible combinations. Away, therefore, with the vender of nostrums, who cannot rank among philosophers and chemists; away with the physician whose incapacity or equivocal qualifications could have been formerly usurped under the garb of Science, and of literary titles. These, and all propagators of evil and errors, cannot stand the test of science, because like truth, science has impressed its features on the physiognomy of its votaries. Science is possessed of its own language, and that of chemistry, which has substituted its nomenclature to the absurd and unmeaning definitions of the old, is the last trait, gentlemen, which distinguishes its adepts from the vulgar or unskilled, as much as it guards its discoveries against any unfounded innovation, and exalts its supremacy, among philosophical sciences, by the language of truth, which belongs to it alone.

Here I conclude, Gentlemen, an imperfect survey of the improvements procured by Chemistry, to Philosophical Sciences, Arts and Manufactures. I may, very justly say, that society has received more real advantages from it, in a few years, than during all the preceding ages of ignorance, or of imperfect knowledge of the Laws of Nature. I could not advert to every interesting view of that Science, within the short space of time I have fixed, and perhaps, fatigued your attention.* With regret thus, I have almost omitted to describe its flourishing cultivation

* In the Philadelphia Laboratories new experiments have been lately instituted, relative to the tremendous effects of the fulminating mercury. The galvanic influence also, that astonishing phenomenon of PERPETUAL MOTION, has been minutely investigated by Prof. Woodhouse, both through the METALLIC PILE and the CHAIN OF CUPS OF VOLTA.

in the Universities and Colleges of this great Republic. The splendid talents of several of their Professors, have still more promoted the sedulous emulation and the distinguished abilities of many students, in their numerous classes. But in the name of your institution, I must notice that in the Professor of Chemistry of this university, our worthy President,* that science has not only gained a strenuous vindicator of its doctrines, but also a liberal inquirer after truth, an elegant, and successful experimenter. —You remember what a great man he has had to contend with; Priestley to whom our science is so much indebted, and whose opinions and experiments are to be consulted, in any of the elementary processes of our Laboratories; Priestley, who commands respect in his Chemical controversies, because, as long as some mysteries in Nature will perplex the Philosopher, he is entitled to the same degree of evidence, which he has exhibited in his doctrines; Priestley, the persecuted friend of Liberty, of Religion, and the model of all social and private virtues. Honored is our Society with such Members; congratulated is the Republic, with such Citizens, and happy is the rising generation with such philanthropic examples, which have already opened to you the Golden Era of SCIENCE and LIBERTY.

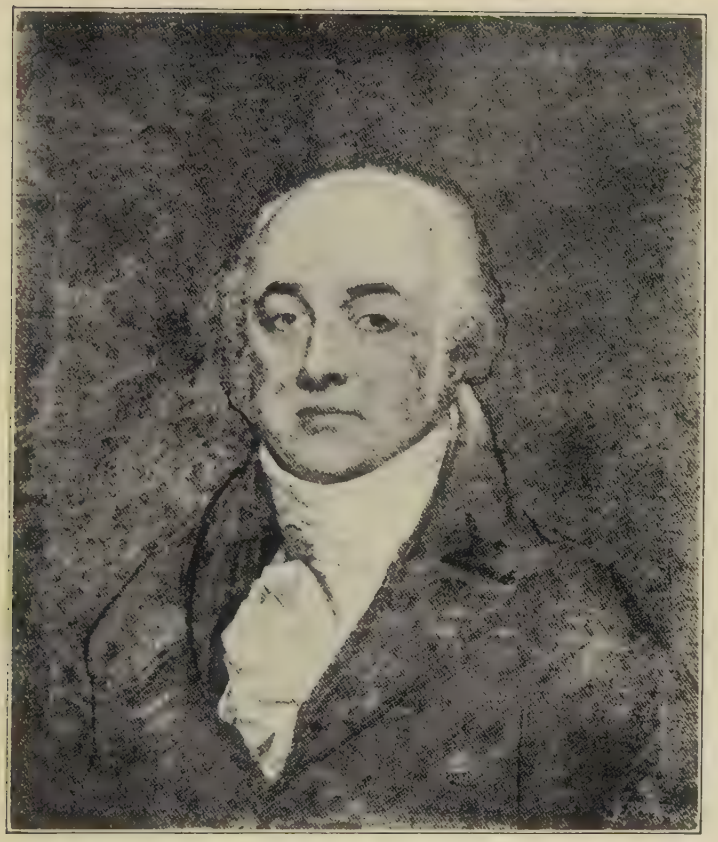
* James Woodhouse.

CHAPTER IV

THE preceding lectures demonstrate the aim of the Society. It is not too much to claim that in this new country very great efforts were being made to promote and to advance chemical thought.

Woodhouse (1770-1809), pioneer in the advocacy of the new chemistry and in the fostering of the science in the United States, was a native of Philadelphia and a graduate of the University of Pennsylvania, where he afterward taught chemistry with credit to himself and his fellows. He is said to have been the first to demonstrate the superiority of Lehigh anthracite coal over the bituminous coal of Virginia for reliability and heating power. He wrote extensively and was the translator and editor of a number of books relating to the science of chemistry, among which is Chaptal's "Chemistry," edited with copious and most interesting notes. A "Chymical Catechism" was the last but not the least of his writings. His "Young Chemist's Pocket Companion" is, in all probability, the first published guide in experimentation for chemical students. The title page and two pages of directions are here introduced.

Woodhouse, as mentioned, was deeply interested in the establishment of the anti-phlogiston theory, and we find that numerous contributions of Joseph Priestley, which appeared in the "Transactions of the American Philosophi-



James Woodhouse

THE

Young Chemiſt's Pocket Companion;

CONNECTED WITH

A Portable Laboratory;

CONTAINING

A PHILOSOPHICAL APPARATUS,

AND A GREAT NUMBER OF

CHEMICAL AGENTS;

BY WHICH ANY PERSON MAY PERFORM AN ENDLESS VARIETY OF AMUSING AND INSTRUCTING EXPERIMENTS;

A INTENDED TO PROMOTE THE CULTIVATION OF THE SCIENCE OF CHEMISTRY.

BY JAMES WOODHOUSE, M. D.
Profeſſor of Chemiſtry in the Univerſity of Pennſylvania, &c.

At preſent every thing that is not denominated Chemiſtry, is but a ſmall part of a ſyſtem of natural knowledge.
PRIESTLEY *on Air.*

Philadelphia:
PRINTED BY *J. H. OSWALD*, NO. 179, SOUTH SECOND-STREET.

1797.

another: it is improper for combustion or animal life; forms neutral salts with the alkalis, and is absorbed by water.

It is used to detect the presence of lime, with which it forms a white insoluble compound.

EXPERIMENT XXXI.

Pour some lime-water into a wine glass, and add a small quantity of the carbonic acid to it, and a white precipitate will be produced.

The carbonic acid unites to the lime, and forms carbonate of lime.

Of the Sulphuric or Vitriolic Acid, or the Oil of Vitriol.

The sulphuric acid is obtained by burning a mixture, composed of one eighth part of nitre, and one of sulphur, in a large chamber lined with lead.

A quantity of water is placed on the floor, to absorb the acid vapors.

The nitre yields pure air to the sulphur, by which means it is converted into the vitriolic acid.

The sulphuric acid is unctuous to the touch; hence it is called the oil of vitriol. It acts strongly on combustible bodies, and is decomposed by combining with them.

EXPERIMENT XXXII.

Put half an ounce of water into a vial, and add one drachm of sulphuric acid to it, and a high degree of heat will be produced, which may be felt by grasping the vial in the hand.

The sulphureous acid gas, or volatile vitriolic acid, is obtained by decomposing the sulphuric acid, with a combustible body.

EXPERIMENT XXXIII.

Put some sweet oil into a vial, and pour upon it a small quantity of the sulphuric acid; connect a glass syphon to the vial, and proceed in the manner directed to obtain oxigenous gas.

cal Society'' for 1799, were discussed at considerable length by him. His replies consisted chiefly of experimental refutations of the statements of the great teacher.

An ANSWER to DR. JOSEPH PRIESTLEY'S ARGUMENTS against the ANTIPHLOGISTIC SYSTEM OF CHEMISTRY, published in the Medical Repository, and a VINDICATION of the PRINCIPLES contained in the 72d Essay of the fourth Volume of the American Philosophical Transactions. By JAMES WOODHOUSE, M. D.

NO. 1.

First. ON THE REVIVAL OF A METALLIC CALX IN INFLAMMABLE AIR.

When the focus of a burning lens is thrown upon a calx of mercury, confined in hydrogenous gas, according to the antiphlogistic theory of chemistry, the oxygen of the calx unites to the hydrogen, and forms water; but, according to Dr. Priestley, the hydrogen enters into the metal, while the oxygen is found mixed with that part of the hydrogenous gas which remains behind.

The Doctor declares, in support of this question, that, in several of his experiments, the pure air, expelled by the heat of the lens from the mercurial calx, was found mixed with the remainder of the inflammable air, as appeared by the test of nitrous air, and by some disagreeable explosions which happened in the process.

Having performed the experiment of the revival of red precipitate in hydrogenous gas, twenty times, without having met with an explosion, I concluded that Dr. Priestley's inflammable air must have been mixed with atmospheric air. I was of this opinion, because I never could detect any pure air mixed with the inflammable air, after

the revival of a mercurial calx in it, by the test of nitrous air.

Since my answer to the Doctor's two pamphlets, I have frequently repeated these experiments, and with the same results as before.

The focus of a lens was thrown upon red precipitate, confined in sixty-two ounce measures of inflammable air, when fifty-two ounce measures of the air disappeared. One measure of the air which remained behind, tried by the test of nitrous air, produced no red appearance, and gave no absorption.

A quantity of the black oxyd of manganese was also exposed to the focus of the lens, in fifty-six ounce measures of inflammable air, when fifty-four ounce measures of the air disappeared the remaining air was azotic, and contained no inflammable or pure air, as appeared by applying a lighted taper to it, and by the nitrous test.

Similar experiments were made in the presence of Dr. Seybert, Dr. Jacobs, and Mr. Lee, gentlemen who are perfectly acquainted with the subject in dispute, and who appeared satisfied, that pure air, expelled by heat from a mercurial calx confined in inflammable air, is not found in that portion of the air which remains behind.

I must, however, admit that I met with an explosion in attempting to revive red precipitate in hydrogenous gas, which I expected contained no pure air. The inflammable gas had been obtained by adding the filings of the bar-iron to water which had been impregnated with sulphurated hydrogenous gas. Upon throwing the focus of the lens upon one drachm of red precipitate, in eight ounce measures of this air, an explosion instantly took place. The pure air, in this case, could not have been given out by the precipitate, for the mercury was not revived; and, as oxygenated muriatic acid * had been formed by the experiment, I cannot account for the explosion until we

* Vide Medical Repository, Vol. III, p. 214.

are better acquainted with the action of iron filings in water impregnated with sulphurated hydrogen gas, and the formation of the oxygenated muriatic acid which is found in the process.

A strong, and in my opinion, a conclusive argument, in support of the opinion that the oxygen of the metallic calx unites to the hydrogen, and forms water, is, that the disappearance of the inflammable air is always in strict proportion to the pure air which the calces contain.

I have shown that iron absorbs twice as much oxygen as copper, and that the calx of iron makes twice as much inflammable air disappear when heated in it by the burning lens; and if a part of the pure air be driven off from the oxyd of manganese by heat, and the oxyd be then exposed to the action of the lens in hydrogenous gas, a very small quantity of the inflammable air will disappear. One drachm of the oxyd of manganese will make twenty-two ounce measures of inflammable air vanish; but the same quantity of manganese, exposed a few hours to a red heat, will make very little of that inflammable air disappear.

In my first answer to Dr. Priestley, I said the manganese was not revived, because no inflammable air could be obtained from it by sulphuric acid and water. It, however, in some cases appears to be revived, and is of a green colour, and the regulus of manganese is described by authors.

I have often heated a large proportion of red precipitate in inflammable air, confined by water, which would rise in the vessel which contained it until the inflammable air disappeared, when it would immediately begin to fall from the pure air yielded by the precipitate.

In one of these experiments, the whole of the inflammable air having vanished, and the pure air from the precipitate having nothing to unite with, was found, unmixed, over the water, and gave, by the test of nitrous air, an absorption of 160.

If the theory of Dr. Priestley was true, that the pure

air of the precipitate was diffused among the inflammable air, an explosion would invariably happen every time that a drachm, or any larger portion of precipitate, was revived in a considerable quantity of hydrogenous gas.

If one drachm of red precipitate was revived in sixty ounce measures of inflammable air, it would give out ten ounce measures of pure air, which would be mixed with forty-eight ounce measures of inflammable air, and which would never fail to cause an explosion.

Secondly. OF THE CALCINATION OF A METAL IN PURE AND ATMOSPHERIC AIR.

According to Dr. Priestley, when a metal is reduced to a calx, in pure or atmospherical air, something which has been called phlogiston is emitted from the metal, which unites with part of the pure air, and converts it into azotic or phlogisticated air. He also says, that the phlogiston, in some cases, unites with a portion of the pure air, and forms fixed air, and that this fixed air is produced by calcining a metal which contains no charcoal.

In all my experiments upon the calcination of bar and cast-iron, and copper, in pure and atmospherical air, I could not find that the air which remained behind was injured. When the focus of the lens was thrown upon sixty grains of the filings of copper, filed for the purpose, confined in sixteen ounce measures of oxygenous gas, twelve ounce measures of the air were absorbed by the metal, which was reduced to a calx. No fixed or phlogisticated air was produced, and the remaining air was perfectly pure.

Dr. Priestley replies to this experiment, that it is impossible to reduce sixteen ounce measures of pure air to four, by calcining a metal in it, and that the remaining four ounce measures should be perfectly pure—for to make dephlogisticated air perfectly pure is hardly possible.

The oxygenous gas which was used was obtained from lead and the sulphuric acid, and gave, by the eudiometer, an absorption of 195 by the nitrous test. Oxygenous gas, equally as pure as this, is seldom met with. It was said to be perfectly pure, because it was supposed the whole of it was devoured by the nitrous test, and that the remaining five hundred parts of a measure consisted of the impurity of the nitrous air, which, Dr. Priestley acknowledges, is very apt to vary in its quality, and very difficult to obtain pure.

Although the Doctor has said, in the Medical Repository, that it is hardly possible to obtain dephlogisticated air perfectly pure, yet, in his pamphlet entitled "The Doctrine of Phlogiston established," speaking of dephlogisticated and nitrous air as the component parts of nitrous acid, "they unite without residuum or so small as not to enter into any computation."* (Page 9). If, then, dephlogisticated air can be obtained to unite with nitrous air, without any residuum, the dephlogisticated air must be perfectly pure.

The oxygenous air which I have since used has been of various degrees of purity, as 180, 178, 169, &c.

The focus of the lens was thrown upon the filings of bar iron, filed for the purpose, confined in fifty-two ounce measures of oxygenous gas, which had been well-washed in lime-water, and was of the purity of 175. Thirty-two ounce measures of the air were absorbed by the metal, which was reduced to a calx. One measure of the remaining air, tried in a eudiometer tube over lime-water, gave an absorption of five hundred parts of fixed air. Another measure of the remaining air, first washed in lime water, gave, by the nitrous test, 170.

Repeating this experiment, by melting the filings of

* In the Transactions of the Royal Society of London, for 1791, p. 215, Dr. Priestley also speaks of dephlogisticated air so pure as to contain no sensible quantity of phlogisticated air.

bar-iron in thirteen ounce measures of oxygenous gas, of the purity of 140, seven ounce measures of the air were absorbed by the metal. One measure of the remaining air, tried in a eudiometer tube over lime-water, gave no absorption, and, consequently, contained no fixed air. With an equal measure of nitrous air, it gave an absorption of 15.

In these experiments no phlogisticated air was generated. The fixed air, formed by melting the iron in pure air, was formed by the coal, which all iron of commerce contains, uniting with part of the pure air. The air which remained behind was more impure than at first, because a portion of the purest part had been absorbed from it by the metals.

Thirdly. OF CARBONIC ACID, OR FIXED AIR.

Dr. Priestley, in order to prove that fixed air is produced without charcoal, mentions that this air is produced by heating charcoal of copper in dephlogisticated air. To this I have replied, that charcoal of copper consists principally of pure charcoal. It is made by passing the steam of alcohol, which consists of hydrogen and carbon, over red-hot copper: the coal is deposited on the copper, while the hydrogen is set at liberty, in the form of hydrogen gas.

The Doctor says, the French chemists have given a much better explanation of this experiment than I have done—but our explanation is exactly the same. The fixed air is formed by the carbon of the charcoal of copper uniting with the dephlogisticated air. My opponent has misunderstood my meaning in explaining the experiment.

Another argument used by Dr. Priestley, to prove that fixed air may be made without coal, is, that large quantities of this kind of air may be obtained from heating a

mixture of iron filings and red precipitate. He declares the experiment has never failed with him, and I say it has never succeeded with me.

If large quantities of fixed air can be formed, by heating the filings of pure bar-iron and red precipitate together, then I will pronounce that fixed air may be made without coal; but I am confident this cannot be done.

From the process which the Doctor has published, to purify iron filings,* it is evident that those he used could not have been very pure, or they would not require to be first heated, then washed in water, and heated again.

Dr. Priestley never mentions whether he used the filings of bar or cast-iron, which is essentially necessary. The filings of pure bar-iron, filed for the purpose, on a clean sheet of paper, exposed to heat with red precipitate, will not yield any kind of air; but cast-iron alone, or mixed with precipitate, will yield both inflammable and fixed air.

One ounce of the borings of cannon, and half an ounce of red precipitate, gave thirty-two ounce measures of air, eleven of which were fixed, and twenty-one inflammable. The fixed air proceeded from the pure air of the precipitate uniting with the charcoal of the cast-iron.

The borings, by analysis, yielded eighteen grains of charcoal to the ounce.

In my opinion, the proofs that fixed air is composed of oxygen and carbon, are as strong as that Glauber's salt is composed of sulphuric acid and soda; for we are not only able to compose this gas at pleasure, but to separate it into its elementary parts.

Mr. Tennant, Dr. Black, and other chemists, have decomposed the carbonic acid, by heating phosphorus and powdered limestone. I have performed the same experiment with success. Forty grains of phosphorus, cut into very small pieces, were mixed with powdered lime-stone,

* Medical Repository, Vol. II, p. 267, first edition.

and introduced into a glass tube, coated with dung and clay. Upon exposing the tube half an hour to red heat, and breaking it when cold, the coal was found mixed with phosphate of lime. The phosphorus united with the oxygen of the carbonic acid of the lime-stone, and formed phosphoric acid, which joined with the lime and made phosphate of lime. The coal of the carbonic acid was deposited among the phosphate of lime.

If fixed air is composed of inflammable and dephlogisticated air,* why is it not obtained by exploding pure air, and the inflammable air from malleable iron?

Speaking on this subject, Dr. Priestley says, "when the inflammable air was from the turnings of cast-iron, there was a considerable quantity of fixed air produced; whereas there was either no fixed air at all, or the slightest appearance of it imaginable, when I made use of inflammable air from malleable iron." †

The reason that the inflammable air, from the turnings of cast-iron, yields fixed air, when fired with dephlogisticated air, is, that it holds coal in solution, which unites with the pure air to form the fixed air, and no fixed air is obtained from the inflammable air from malleable iron, because it contains but a very minute portion of coal. If fixed air can be formed by exploding only one kind of inflammable air with pure air, there must be some foreign substance in the inflammable air; and what can this be if it is not coal? for bar and cast-iron differ from each other only in the quantity of coal they contain—an ounce of bar-iron yielding but half a grain of coal, and the same quantity of cast-iron, as I have said before, eighteen grains.

The Doctor says, when any substance, known to contain oxygen, is heated in inflammable air, fixed air is found.

* "We say that fixed air consists of inflammable and dephlogisticated air." Vide Doctrine of Phlogiston established, p. 61.

† Transactions of the Royal Society of London for 1791, p. 221.

(Considerations on the "Doctrine of Phlogiston," part first, p. 25.) In the second part of the same pamphlet (p. 24), he informs us, he sometimes gets fixed air. In the "Medical Repository," vol. II. p. 164, first edit. he mentions that no sensible quantity of fixed air is procured in this process. If red precipitate is heated in inflammable air, from malleable iron, the result will be uniform; no fixed air will be generated, but it will be invariably made, if the mercurial calx is revived in carbonated inflammable air, from the pure air of the precipitate uniting with the carbon held in solution in this gas. If fixed air was composed of pure and inflammable air, it ought always to be obtained in this process.

When the focus of a burning lens is thrown upon two drachms of red precipitate, in thirty-two ounce measures of inflammable air, from malleable iron, twenty-two ounce measures of the air will disappear; but when three drachms of the same precipitate are heated in thirty-six ounce measures of carbonated inflammable air, from the flowers of zinc and coal, which has been well washed in lime-water, but two ounces of the air will vanish. In the first case no fixed air will be obtained, but in the second there will be a great production of this gas.

Fourthly. OF FINERY CINDER, OR THE SCALES OF IRON.

Large quantities of carbonated inflammable air, mixed with a portion of fixed air, are produced by heating finery cinder and charcoal together, though both may have been previously exposed to ever so high a degree of heat. In considering what takes place in this process, we must call to our aid the decomposition of water, the clue which leads us through all the labyrinths of the antiphlogistic system of chemistry. The carbonated inflammable air is formed by the hydrogen of the water, which is supplied by the

finery cinder dissolving part of the coal, while the oxygen of the water and finery cinder, uniting with another part of the coal, make the fixed air.

We are under a necessity of admitting the presence of water in the finery cinder. It cannot be in the coal, where Berthollet, Fourcroy, and other chemists find it; for, in my experiments, the coal has ceased to yield air, and, consequently, could not contain water.

In my first reply to Dr. Priestley, I said the iron was not revived. I find, however, after the finery cinder is exposed to heat with charcoal, it will yield inflammable air when mixed with sulphuric acid and water. The iron, then, must be in a revived state. I was deceived, by supposing that a calx of iron could not be revived in a degree of heat less than that at which it fuses. Cast-iron melts at 130 deg. and my finery cinder was exposed to but 24 deg. of Wedgwood's thermometer.

I consider the arguments of the Doctor, relating to finery cinder and charcoal, as a complete refutation of the doctrine of the French chemists, relating to this subject, though I do not think the new theory is essentially affected by anything which he has advanced. Part of the weight of the scales of iron is certainly owing to water. The advocates of the antiphlogiston system have overlooked the agency of this fluid in the finery cinder.

If, in future, I find that no more fixed air is obtained from the scales of iron and charcoal than from coal and water, I will agree with my opponent, that they contain but a very small quantity of oxygen, or none at all.

Fifthly. OF THE PRECIPITATION OF ONE METAL BY ANOTHER.

Inflammable air is produced, when zinc is used to precipitate lead from a solution of sugar of lead, or iron from its solution in the muriatic acid. The French chemists ap-

pear to be unacquainted with this circumstance, as well as with many other important discoveries made by my illustrious opponent. Mrs. Fulhame, who has written on the precipitation of metals, was ignorant of the fact.

I have obtained inflammable air,

1st, From the filings of zinc, and a solution of the sulphates of iron and copper.

2ndly, From the filings of bar and cast-iron, and the sulphate of copper.

3dly, From copper, precipitated from blue vitriol by zinc, which was washed in water until the water would not precipitate muriated barytes, mixed with the filings of zinc. And,

4thly, From the oxyd of copper, precipitated from blue vitriol by caustic pot-ash, and the filings of zinc and iron.

One scruple of the filings of zinc, and eight ounces of a saturated solution of blue vitriol, in eleven hours yielded no air: a second scruple being added, in the same space of time no air was obtained: upon adding the third scruple, in forty-eight hours one fourth of an ounce measure of inflammable air was produced. The precipitated copper weighed forty-five grains. It was not until after the eighth scruple was used that the air was obtained in any quantity.

Half an ounce of the filings of zinc, and eight ounces of a saturated solution of green vitriol, gave, in nineteen days, forty ounce measures of inflammable air.

One ounce of the filings of zinc, and eight ounces of a saturated solution of blue vitriol, gave, in ten days, sixty-four ounce measures of inflammable air.

The filings of iron afford but a small quantity of inflammable air, compared to zinc, when mixed with the sulphate of copper. One ounce of the borings of cannon, and eight ounces of a solution of blue vitriol, in four days produced but six ounce measures of inflammable air. A considerable degree of heat is generated in this process.

In these experiments, when the precipitant is added in

small portions, the operation which takes place is its solution without any production of air. The oxygen of the dissolved mineral unites with the precipitant, forming an oxyd, which is immediately dissolved by the acid. The precipitated metal is in a revived state, from the loss of its oxygen. When the precipitant has robbed the dissolved metal of the whole of its oxygen, it decomposes the water by means of part of the acid to which the dissolved metal was united. The oxygen of the water, united to the precipitant, converts it into a calx, which is dissolved by the acid, while the hydrogen of the water is set at liberty.

When inflammable air is obtained from copper, precipitated by iron, mixed with the filings of zinc, the zinc robs the precipitated copper of its oxygen; but as there is not a sufficiency of oxygen in the copper to oxyde the zinc completely, it begins to decompose the water. When a solution of the sulphate of copper is precipitated by zinc, the whole of the precipitated metal is not in a revived state—part of it is calcined.

Upon throwing the focus of a burning lens upon some of this precipitated copper in inflammable air, it made four ounce measures of the air disappear.

Sixthly. OF THE AIR CONTAINED IN THE PORES OF CHARCOAL, WHICH HAS BEEN EXPOSED TO A RED HEAT.

Dr. Priestley says, that charcoal contains azotic gas, but I have always found it to be atmospherical air. One measure of the air obtained from coal, by means of water, gave, with the nitrous test, an absorption of 90.

Woodhouse, in his opposition to Priestley, was always respectful and fair, but there were those in this country who were disposed to belittle the contributions of the

great philosopher. Among these was John Maclean, Professor at Princeton. Woodhouse felt called upon to answer one of his attacks upon Priestley. The reader may form an idea of the character of Woodhouse's disputations from that communication:

A Letter to DR. JOHN MACLEAN, Professor of Mathematics and Natural Philosophy in Princeton College, New Jersey, by JAMES WOODHOUSE, M.D.

SIR,

As there are several assertions, in your examination of Dr. Priestley's considerations on the doctrine of phlogiston and decomposition of water, relating to some important parts of chemistry, which are absolutely erroneous, I think it necessary to call your attention to the subject.

As you wrote your dissertation expressly to prevent the youth of Princeton college from falling even into temporary delusion, and as public controversy is always favourable to the cause of truth, you can have no rational objection to this letter.

A judgment may be formed how well you have accomplished your purpose, and what right you have to condemn the experiments of Dr. Priestley in the authoritative manner you have done, having made none yourself, from the following particulars. You are not yet, Doctor, the conqueror of this veteran in philosophy.

You agree with the French chemists, that turbith mineral is an oxyde of mercury, and have asserted, that any substance into which it may be converted by a red heat, does not require any addition to constitute it a metal.

Now, the very contrary of this is true; for we have the most conclusive proofs, that turbith mineral is not an oxyde, but a sulphate of mercury.

1st. If pure turbith mineral is exposed to a red heat,

in a long glass tube, a quantity of the sulphate of mercury, of a white colour and strong acrid taste, sublimes from it, and adheres to the sides of the vessel.

2dly. If a solution of caustic pot-ash is boiled upon the turbith, it suffers a considerable loss in weight, and loses its bright yellow colour, and is converted into a calx of the colour of brick-dust. The solution, by spontaneous evaporation in the open air, will yield crystals of vitriolated tartar.

3dly. If distilled water is boiled upon the turbith, and renewed from time to time, the water will always precipitate a solution of muriated barytes.

These experiments incontestibly prove, that turbith mineral is not an oxyde, but a sulphate of mercury.

It is no objection to this opinion, that the turbith, when exposed to a red heat, yields oxygenous gas, and that running mercury is obtained; for the sulphuric acid leaves one part of it and joins to another, which sublimes in the form of a white salt. That part which the acid deserts is converted into an oxyde, is revived without addition, and yields pure air.

This sulphate of mercury is the supposed calx to which Dr. Priestley refers. It is sometimes obtained of a red colour, owing to some substance which deprives a part of the sulphuric acid of its oxygenous gas, and converts it into sulphur, which, uniting with the fluid mercury, sublimes in the form of cinnabar, and gives the whole of the salt a red colour.

This is what you ought to have ascertained, if you intended to have acquired the character of an accurate investigator.

Your next assertion is, that red lead contains more oxygene than a calx of iron, from which circumstance you suppose, that the former calx oxygenates the muriatic acid, and the latter does not, as it contains but a small quantity of pure air.

Your words are, "It certainly does not follow, because muriatic acid can separate a certain portion of oxygene from lead, when this is combined with a *great quantity* of this substance, that it should likewise separate oxygene from iron, when this is united to a comparatively *small quantity.*"

You will grant, that when a pure metallic calx is heated in hydrogenous gas, that the oxygene of the calx unites to the hydrogene, and forms water; consequently, those calces which make the greatest quantity of inflammable air disappear, contain the most oxygene.

Having heated one drachm of red lead by a burning lens, eleven inches in diameter, in hydrogenous gas, obtained from the sulphuric acid, diluted with water, and malleable iron, and which had been well washed in lime-water, it made ten ounce measures of the air disappear.

One drachm of the precipitate of iron, from green vitriol by ammoniac, or a solution of mild pot-ash and the common rust of iron, heated in the same manner, made thirty-six ounce measures of the air vanish. One drachm of the filings of bar-iron melted in oxygenous gas, absorbed twenty-six ounce measures of this air.

One hundred grains of well dried red lead, according to Lavoisier, contain 89.93 metal, and 7.64 oxygene; and the same quantity of the precipitate of iron, from green vitriol, by caustic pot-ash, according to Gadolin, contains 58.48 metal, 15.91 oxygene, 25.39 water. One hundred parts of the yellow calx of iron, according to Lavoisier, 68.66 metal, and 32.34 oxygene.

Your opinion, then, according to these experiments, in regard to the quantity of oxygene which the calces of iron and lead contain, is void of foundation.

The true reason that red lead would oxygenate the muriatic acid, and that a calx of iron will not, is that the former readily gives its oxygene to the acid, and the latter does not, owing to a difference in the elective attractions

subsisting between the acid, oxygene, and the two metals.

It is evident, that the oxygenation of the muriatic acid does not merely depend upon the quantity of oxygene contained in the calx; for one drachm of manganese, which has been exposed to a red heat, and parted with most of its pure air, will oxygenate the acid to a greater degree than an ounce of the calx obtained from boiling a solution of caustic alkali upon turbith mineral, which contains thirty times the quantity of oxygenous gas.

You have also declared that Dr. Priestley is mistaken, in saying that finery cinder will not acquire rust, and assert that it contracts rust sooner than common iron.

To determine this question, a quantity of the scales which the blacksmiths strike off from a red hot iron, reduced to an impalpable powder, were exposed to the action of the air more than twelve months, and were sprinkled with water several hundred times, were as free from rust as when first exposed.

The rust which finery cinder appears to contract is owing to iron-filings, with which it is frequently mixed. The pure scales never will acquire rust; for, when bar-iron is converted into finery cinder, it parts with the small quantity of coal it contained, and absorbs oxygene and water.

You have answered the Doctor, on this part of the controversy, by informing him, that inflammable air is a constituent part of other bodies besides water; that hydrogene is retained, with greater force, by coal; that unglazed earthen vessels absorb moisture; and, lastly, you tell him in what manner the experiment ought to have been performed, and declare that it is of no value, as reported in his experiments on different kinds of air.

I have repeated this famous experiment, and the result is exactly as stated by Dr. Priestley.

One ounce of the scales of iron, and the same quantity of charcoal, were separately exposed, in two covered cruci-

bles, in an air-furnace, well supplied with fuel, for five hours. They were then taken out of the fire, and mixed, while *red-hot,* in a *red-hot* iron mortar—were poured upon a *red-hot* piece of sheet-iron, and instantly put into a red-hot gun barrel, which was fixed in one of Lewis's black lead furnaces, and communicated with the worm of a refrigeratory, a part of a hydro-pneumatic apparatus. Immediately after, luting the gun barrel to the worm, one hundred and forty-two ounce measures of inflammable air came over in torrents, mixed with a tenth part of carbonic acid gas.

This experiment has puzzled every person to whom it has been mentioned.

For my part, I do not think it affects the antiphlogistic system; for the scales of iron contain water, and retain it in so obstinate a manner as not to part with it upon the application of heat; but when coal is added to the finery cinder, it takes away the water, by having a greater affinity to it than to the calx of iron. The coal decomposes this water; its oxygene unites to part of the coal, and forms the carbonic acid; while its hydrogene is separated, dissolves another part of the coal, and forms the carbonated hydrogene gas.

Dr. Priestley's explanation of this experiment is very unsatisfactory; for he says, the phlogiston of the charcoal contributes to revive the iron; but the Doctor ought to have remembered, that an oxyde of iron cannot be revived in one of Lewis's small black lead furnaces.

There are other substances besides finery cinder, which, when mixed with coal which has ceased to yield air, give inflammable air in large quantities. It may be obtained from any precipitate of iron or zinc, or from the flowers of zinc mixed with red-hot coal; and the hydrogen gas procured will always be in proportion to the water which the calces contain, and the metals will not be revived.

Should you consider the objections of Dr. Priestley once

more, and advance nothing but what is founded upon your own experiments, you may hear from me again; and I promise not to be the first to drop the subject.

Mere assertions only serve to fix errors deeply in the mind, and do not advance the cause of truth.

Hoping that I do not intrude upon the precious moments of your time, which is *more agreeably, and, perhaps, more usefully employed,* than in discussing this subject,

I am, Sir, with consideration,

Yours, &c.

JAMES WOODHOUSE.

DR. JOHN MACLEAN.

Woodhouse's ability as an investigator and an experimenter are demonstrated in the subjoined essay:

OBSERVATIONS ON SEVERAL METHODS OF OBTAINING OXYGENOUS GAS IN A VERY PURE STATE.

A cheap and easy method of obtaining oxygenous gas, perfectly pure, in large quantities, has long been a desideratum with chemists. The azotic air, with which this gas is generally contaminated, renders it improper to be used in many delicate operations. In decomposing hydrogen gas, carbonated hydrogen gas, and the oxyds of carbon, of the same quality, in the eudiometer of Volta, different results will happen, unless the oxygenous air be exactly of the same strength, or unless it be completely freed of azotic air, which produces the variations in the experiments.

Dr. Priestley, who has had more experience in pneumatic philosophy than any other person, says, "to make dephlogisticated air perfectly pure is hardly possible;"* and the French and British philosophers recommend but

* Medical Repository, Vol. III, p. 122.

one substance, the hyperoxygenated muriate of pot-ash, for this purpose. This article is at present too dear in Europe to be much used by the chemist, and it cannot be procured in the United States.

Reflecting upon this subject, it occurred to me that an oxyd of mercury, prepared by boiling a solution of pot-ash upon turbith mineral, would afford the air in the state required.

Turbith mineral is made by boiling the sulphuric acid upon mercury, until a dry white salt is formed, upon which a large quantity of boiling water is poured. A part of the oxygen of the acid unites to the mercury, and converts it into an oxyd, which is dissolved by part of the acid forming sulphate of mercury. Another part of the oxygen of the acid seizes hold of part of its sulphur, and makes sulphureous gas, or volatile vitriolic acid, which escapes.

The hot water dissolves the sulphate of mercury, containing an excess of acid, and leaves a substance behind, of a yellow colour, which is called turbith mineral.

A considerable portion of sulphuric acid still adheres to this preparation, of which it may be freed by boiling it in a solution of pot-ash, but which cannot be separated by boiling it ever so long in water.

The pot-ash will unite to the sulphuric acid, and form sulphate of pot-ash, or vitriolated tartar; while the mercury will be left in the form of an oxyd of a brown colour. The sulphate of pot-ash being soluble in water, may be washed away from the oxyd of mercury.

The agents employed in manufacturing this article are sulphuric acid, or oxygen and sulphur, mercury, pot-ash, and water; neither of which, except the last, contains any azotic air, and this adheres to and is thrown away with the water. One ounce of an oxyd of mercury, prepared in this manner, submitted to a red heat in an iron tube, yielded forty cubic inches of oxygenous gas.

This air was examined by phosphorus, by the nitrous test, and by exploding it with hydrogen gas from sulphuric acid, diluted with water, and malleable iron.

A piece of phosphorus, the size of a duck-shot, was stuck upon the end of an iron wire, introduced into one hundred parts of the gas, confined over water in an eudiometer. Upon approaching a lighted taper near the phosphorus, it immediately inflamed, an absorption of the water took place, and but two hundred parts of a measure of azotic air remained.

Four measures of nitrous air, from diluted nitric acid and copper, were added to one of this oxygen gas, in an eudiometer. The first gave an absorption of 120, the second 134, the third 150, and the fourth 70.

Two cubic inches of this oxygen air, and four of hydrogen gas, exploded by the electric spark, in the eudiometer of Volta, left one-fourth of a cubic inch of air, which was principally azotic.

The materials and apparatus employed in making this oxyd of mercury are not very expensive, nor are the operations difficult or troublesome.

The acid may be boiled upon the mercury in an oil-flask, and the boiling water may be poured upon the dry sulphate of mercury in powder, in a queen's ware wash-hand basin. When the sulphate of mercury, containing an excess of acid, is washed away, the remaining sulphate of mercury, or turbith mineral, may be digested a few hours in a hot solution of pot-ash, which will free it from every particle of the sulphuric acid. The greatest part of the mercury may be saved when the air is obtained from the oxyd, for it will be found in a revived state in the iron tube.

If turbith mineral, which has not been digested with a solution of pot-ash, is exposed to a red heat in a closed vessel, having a syphon in its mouth, it will afford oxygen gas, and a quantity of the sulphate of mercury of a white colour will sublime from it, adhere to the side of the vessel,

and fill up the bore of the syphon; hence it is best to use the oxyd prepared from this salt by means of pot-ash.

A second method which was tried to procure oxygen gas in a pure state, was by exposing the leaves of vegetables to the influence of solar light, in pump water, which generally contains a portion of carbonic acid, or in boiled or distilled water, impregnated with this air.

A small handful of the green leaves of any plant will yield six, eight, or ten cubic inches of oxygenous air, when exposed to the action of the sun in one hundred and forty cubic inches of the pump water of this city; and, provided the air was pure, as we would expect *a priori,* for it arises from the decomposition of the carbonic acid, which contains no azote, it could be obtained in sufficient quantities, in the summer season for chemical experiments. A small handful of the healthy leaves of Datura stramonium, Phytolacca decandra, and Polygonum aviculare, were separately exposed, eight hours, to the light of the sun, in one hundred and twenty cubic inches of pump water, which was known to contain carbonic acid, and in the same quantity of boiled and distilled water, impregnated with this air.

From six to eight cubic inches of oxygen gas were obtained from the leaves of each plant.

Phosphorus, inflamed over water, in one hundred parts of this gas, absorbs 70 parts: the remaining 30 parts were azotic air, as appeared by the nitrous test.

Three measures of nitrous gas were added to one of this oxygenous air. The first gave an absorption of 100, the second 105, and the third 0.

Two cubic inches of the oxygen gas, exploded in the eudiometer of Volta, with four cubic inches of hydrogen gas, left two cubic inches of inflammable and azotic air.

From whence came this azotic air with which the oxygen gas was so highly contaminated? Was it separated by the

light and heat of the sun from the water, or was it excreted by the leaves?

As it is difficult, and perhaps impossible, to throw off all the azotic air which water contains, by boiling or distilling it; and as the Galvanic influence, passed through boiled and distilled water, always separates a considerable quantity of azotic gas from it, there is no doubt but that this air was contained in the water.

The contamination, however, is so great, that the oxygen gas, procured in this manner, cannot be used in nice experiments.

A third method which presented itself of obtaining pure oxygen gas was from the oxyd of manganese.

This metallic substance often contains carbonate of lime, always azotic gas, and frequently carbonic acid, which it absorbs from the atmosphere.

Two ounce measures of sulphuric acid, diluted with a pint of water, were boiled one hour upon half a pound of the oxyd of manganese, in fine powder, in a glass retort, the mouth of which entered a jar, filled with water, and placed upon the shelf of an hydropneumatic tub. Fifty cubic inches of air were obtained, forty-six of which were carbonic acid gas, and four azotic air. The acid being washed away from the oxyd, a portion of it was exposed to heat in an iron tube, and it afforded oxygen air perfectly free from carbonic acid gas.

Phosphorus, placed in twenty cubic inches of this air, over water, absorbed nineteen inches of it, and left one cubic inch of azotic gas.

The same quantity of oxygen air, from the same kind of manganese, upon which no acid had been boiled, treated in the same manner, left two cubic inches of azotic air.

The oxygen air from manganese, upon which no acid had been boiled, treated in the same manner, left one cubic inch of hydrogen and azotic gas.

Four measures of nitrous air were added to one of the

oxygenous gas, from the purified manganese: the first gave an absorption of 120, the second 145, the third 115, and the fourth 15.

Four measures of the same air were added to one of the oxygen gas, from manganese not boiled in the acid. The first gave an absorption of 120, the second 130, the third 100, and the fourth 0.

Phosphorus, fired in 100 parts of the oxygen gas, from the purified manganese, left three hundred parts of a measure of azotic air.

One hundred parts of the oxygen gas, from the manganese to which no acid had been added, treated in the same manner, left five parts of azotic air.

The quantity of azotic gas contained in this oxyd of manganese was very small, and the greater part of it was thrown off with the carbonic acid, by the sulphuric acid and water.

Exposing the purified manganese, in an earthen retort, to a red heat, the oxygen air obtained was found to contain ten per cent. azotic gas. A part of the atmospheric air entered the pores of the heated retort, and mixed with the oxygen gas: hence an earthen vessel never should be used when it is necessary to have oxygen air very pure.

Although many writers say that oxygen gas can be procured perfectly pure from the hyperoxygenated muriate of pot-ash, yet I never could obtain it of a higher degree of purity than the air from the oxyd of mercury, from turbith mineral by pot-ash, or from manganese boiled in sulphuric acid and water. Upon comparing the oxygen gas from these three substances, no difference could be observed between them. The oxygen air, from the oxy-muriate of pot-ash, devoured nearly four measures of nitrous gas, yielded two per cent. azotic air, when phosphorus was fired in it, and left one-fourth of a cubic inch of azotic and hydrogen gas, when two cubic inches of it were exploded with double the quantity of hydrogen gas.

My experiments on this subject exactly coincide with those of Dr. Priestley, and it is highly probable that oxygen gas never has been obtained in a state of perfect purity.

There are several other oxyds of mercury, besides that from turbith mineral, from which oxygen air may be obtained very pure, as mercurius precipitatus per se, the oxyd from red precipitate, boiled in a solution of pot-ash, to free it from every particle of nitric acid it may contain; but these preparations are very expensive.

Having examined two hundred cubic inches of the oxygen air from nitre, as it was produced, the first few cubic inches were found to be mixed with six per cent. of azotic air, which gradually increased until it amounted to forty parts in the hundred, which the last portions contained; so that the air from this salt scarcely deserves the name of oxygen gas.

Benjamin Silliman, the elder, spent the winter of 1802-1803 and that of 1803-1804 in Philadelphia, for the purpose of attending lectures on chemistry and allied subjects. He has left a most interesting word-picture of Woodhouse:

The lectures on Chemistry by Dr. James Woodhouse were given in a small building in South Fifth Street, opposite to the State-House Yard. Above, over the laboratory, was the Anatomical Hall. Neither of these establishments was equal to the dignity and importance of the Medical School, and the accommodations in both were limited; the lecture-rooms were not capacious enough for more than one hundred or one hundred and twenty pupils and there was a great deficiency of extra room for the work, which was limited to a few closets. The chemical lectures were important to me, who had as yet seen few chemical experiments. Those performed by Dr. Woodhouse were valuable, because every fact, with its proof, was an acqui-

sition to me. The apparatus was humble, but it answered to exhibit some of the most important facts in the science; and our instructor delighted, though he did not excel, in the performance of experiments. He had no proper assistant, and the work was imperfectly done; but still it was a treasure to me. Our Professor had not the gift of a lucid mind, nor of high reasoning powers, nor of a fluent diction; still, we could understand him, and I soon began to interpret phenomena for myself and to anticipate the explanations. Dr. Woodhouse was wanting in personal dignity, and was, out of lecture hours, sometimes jocose with the students. He appeared, when lecturing, as if not quite at his ease, as if a little fearful that he was not highly appreciated,—as indeed he was not very highly.

In his person he was short, with a florid face. He was always dressed with care; generally he wore a blue broadcloth coat with metal buttons; his hair was powdered, and his appearance was gentlemanly. His lectures were quite free from any moral bearing, nor, as far as I remember did he ever make use of any of the facts revealed by chemistry, to illustrate the character of the Creator as seen in his works. At the commencement of the course he treated with levity and ridicule the idea that the visitations of the yellow fever might be visitations of God for the sins of the people. He imputed them to the material agencies and physical causes—forgetting that physical causes may be the moral agents of the Almighty. His treatment of myself was courteous. I dined with him in his snug little bachelor's establishment,—for he had no family, and a matron housekeeper superintended his small establishment. I should add respecting his lectures that they were brief. He generally occupied a third or a fourth of the hour in recapitulating the subject of the preceding lecture, and thus he advanced at the rate of about forty or forty-five minutes in a day.

At the commencement of my first course with him, in

1802, he had just returned from London, where he had been with Davy and other eminent men. He brought with him a galvanic battery of Cruickshank's construction,—the first that I had ever seen,—but as it contained only fifty pairs of plates, it produced little effect. Dr. Woodhouse attempted to exhibit the exciting effects of Davy's nitrous oxide, but failed for want of a sufficient quantity of gas, and the tubes were too narrow for comfortable respiration. He did not advert to these facts, but was inclined to treat the supposed discovery as an illusion. I had afterwards, at New Haven, an opportunity to prove that there was no mistake, and that Davy had not overrated the exhilarating effects of the gas when respired conveniently and in proper quantities,—three or four quarts to a person of medium size, inhaled through a wide tube. An amusing occurrence happened one day in the laboratory. Hydrogen gas was the subject, and its relation to life. It was stated that an animal confined in it would die; and a living hen was, for the experiment, immersed in the hydrogen gas with which a bell-glass was filled. The hen gasped, kicked, and lay still. "There, gentlemen," said the Professor, "you see she is dead"; but no sooner had the words passed his lips, than the hen with a struggle overturned the bell-glass, and with a loud scream flew across the room, flapping the heads of the students with her wings, while they were convulsed with laughter. The same thing might have occurred to anyone who had incautiously omitted to state that this gas is not poisonous, like carbonic acid, but kills, like water, by suffocation.

The death of Dr. Woodhouse took place in 1809, I suppose from apoplexy. He was found dead in his bed. He had a short neck, and was of a full, sanguineous habit. The chemistry of that period—that of my attendance on the lectures of Dr. Woodhouse, more than half a century ago—had not attained the precision which it now has.

The modern doctrine of definite proportions or equivalent proportions was then only beginning to be understood; the combining proportions of bodies were generally given in centesimal numbers, and thus the memory was burdened, and with little satisfaction. The modern analysis of organic bodies was then hardly begun. Galvanism had indeed awakened Europe, and progress had been made toward those interesting developments which have filled the world with astonishment; but their era was several years later. We may not, therefore, impute to a professor of that period the deficiencies which belonged to that stage of the science.

I had not reason to regret that I attended on the lectures of Dr. Woodhouse. He supplied the first stepping stones by which I was enabled at no distant day to mount higher.

Benjamin Rush wrote of Woodhouse:

He was a neat experimenter, but averse from principles in chemistry . . . . his lectures contained nothing but facts. He was an open and rude infidel. (Rush, A Memorial, printed in 1905.)

A third contemporary remarks:

Upon Woodhouse's appointment to the professorship at the University of Pennsylvania, he began immediately to prepare himself for the duties of his new and promising career. He became, in a short time, so expert and successful an experimenter, as to receive from Dr. Priestley, who had just arrived in the United States, very flattering comments on his dexterity and skill. That distinguished gentleman, on seeing him engaged in the business of his laboratory, did not hesitate to pronounce him equal, as an experimenter, to anyone he had seen in either England or France. At times, his devotion to chemistry and the labor he sustained in the cultivation of it were perfectly

marvellous—not to say preternatural. During an entire summer (one of the hottest I have ever experienced), he literally lived in his laboratory, and clung to his experiments with an enthusiasm and persistency which at length threw him into a paroxysm of mental derangement. He even believed, and, on one occasion, proclaimed, in a company of ladies and gentlemen, that, by chemical agency alone, he could produce a human being.

The special object of his experiment at that time was the decomposition and recomposition of water. The agent employed in his processes was, of course, caloric. And no alchemist in pursuit of the alcahest, or the philosopher's stone, ever labored in his vocation with a wilder enthusiasm, a more sublimated intensity, or a perseverance more stubborn, than he did, immersed in a temperature intolerable to any human being possessed of natural and healthful sensibility.

As already mentioned, the weather was almost unprecedentedly hot; and his laboratory was in sundry places perpetually glowing with blazing charcoal, and red-hot furnaces, crucibles, and gun-barrels, and often bathed in every portion of it with the steam of boiling water. Rarely, during the day, was the temperature of its atmosphere lower than from 110° to 115° of Fahrenheit—at times, perhaps, even higher.

Almost daily did I visit the professor in that salamander's home, and uniformly found him in the same condition—stripped to his shirt and summer pantaloons, his collar unbuttoned, his sleeves rolled up above his elbows, the sweat streaming copiously down his face and person, and his whole vesture drippingly wet with the same fluid. He, himself, moreover, being always engaged in either actually performing or closely watching and superintending his processes, was stationed for the most part in or near to one of the hottest spots in his laboratory.

My salutation to him on entering his semi-Phlegethon

of heat not infrequently was: "Good God, doctor, how can you bear to remain so constantly in so hot a room? It is a perfect purgatory!" To this half interrogatory, half exclamation, the reply received was usually to the same purport. "Hot, sir—hot! do you call this a hot room? Why, sir, it is one of the coolest rooms in Philadelphia. Exhalation, sir, is the most cooling process. And do you not see how the sweat exhales from my body, and carries off all the caloric? Do you not know, sir, that, by exhalation, ice can be produced under the sun of the hottest climates?"

Such was the professor's doctrine; nor have I the slightest doubt of his belief in its correctness. So deep is the hallucination in which alchemy first, and afterward chemistry, its lineal descendant, have, in many cases, involved the minds of their votaries and rendered them permanently wild and visionary in their action. It is not, I think, to be doubted that alchemy and chemistry have deranged a greater number of intellects than all other branches of science united. Even at the present day it is hardly short of lunacy to contend, as many chemists do, that chemical and vital forces are identical.

Dr. Woodhouse, phlegmatic and saturnine as he usually was, possessed and displayed at times some of the crotchets which characterize genius. His didactic lectures rarely occupied, each of them, more than forty minutes—and often not near so much. And when interrogated on the subject, the reason he rendered for such brevity was, that "no man could dwell, in discussion, on a single topic more than five minutes without talking nonsense."

CHAPTER V

THE arrival of Joseph Priestley in America, in 1794, and his frequent presence among the men of science of that day, greatly stimulated scientific studies. The minutes of the American Philosophical Society show that on various occasions he was present at the ordinary meetings of the Society, which would mean that men like Woodhouse and others probably had frequent intercourse with him, and thus, learning to understand the man in his true nature, there was no hostility whatsoever to him. Benjamin Franklin had made the most strenuous efforts to have Priestley locate in the City of Brotherly Love. He had been his friend in England. He spoke of him as the "honest heretic," and it was Franklin who had very materially aided him in the publication of his "History of Electricity." Some of his most ardent friends were also, at the time, holding professional chairs in the University of Pennsylvania, and he, himself, had been invited to occupy the Chair of Chemistry which Woodhouse later accepted. This fact is made quite evident from the letters of Priestley addressed to Dr. Benjamin Rush:

NORTHUMBERLAND, Nov. 3, 1794.

DEAR SIR: I thank you for your kind hint respecting the professor of chemistry; but you will excuse me if I feel a reluctance to comply with it. I cannot appear as a

candidate but if the place was offered to me, I would do my best to discharge the duties of it. The first year, indeed, I should lie under great disadvantage, but so must any other person suddenly called to a new employment.

I am not, however, at all anxious about this business, hoping we shall succeed in establishing a College in this place; and it will be more convenient to me (to) be employed here, than in Philadelphia; tho' a call to spend some time, every year, in that place would not, I acknowledge, be ungrateful to me.

Besides the letter you mention, I took the liberty to write you another, about a house I propose to build here, and other matters.

With gratitude and esteem, I am,

Dear Sir, yours sincerely,

J. PRIESTLEY.

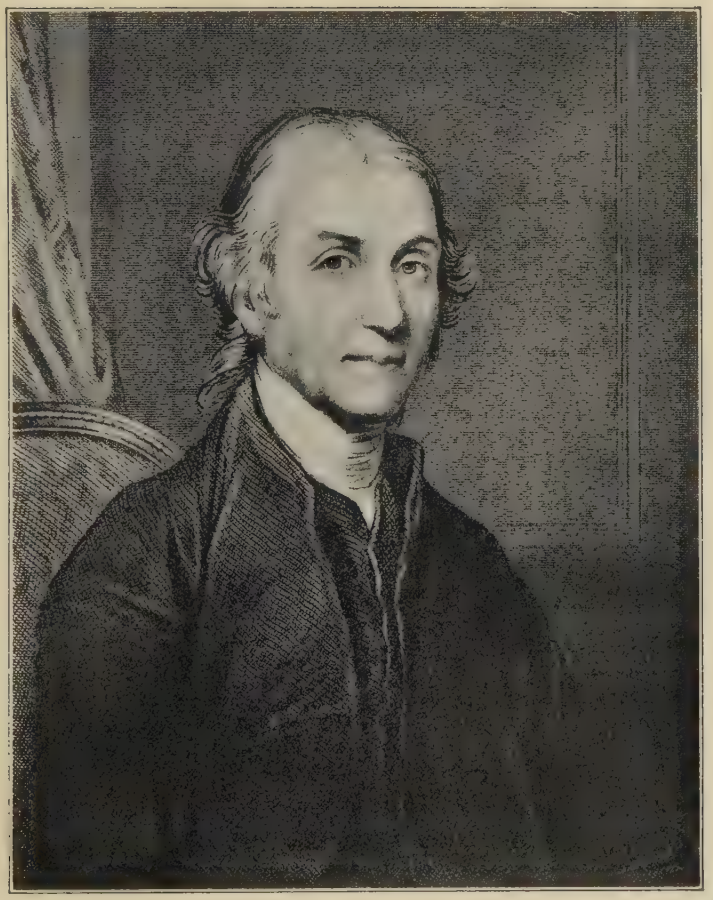
DOCTOR RUSH, PHILADELPHIA.

NORTHUMBERLAND, Nov. 11, 1794.

Dear Sir: I hope you will excuse my weakness (for such you will consider it) when, after giving you reason to expect that I would accept the professorship of Chemistry, if it was offered to me, I now inform you that I must decline it.

On the receipt of your obliging letter, I was determined to accept of it, and in my own mind had every arrangement for that purpose. But when I began to consider the difficulty and irksomeness of a journey to Philadelphia at this time of the year, and especially the obligation I should be under of spending four months of every year from home, my wife in the house by herself, my heart failed me.

This, in fact, is my only objection, but it is an insuperable one. I am truly sensible of the honour that is done me by the invitation, and beg that you would express it for me to all the persons concerned. Nothing could have been so pleasing to me as the employment, and I should



JOSEPH PRIESTLEY

have been happy in your society, and that of other friends in the Capital, and, what I have much at heart, I should have an opportunity of forming an Unitarian congregation in Philadelphia. But the considerations as mentioned, and that of my time of life, lead me to continue where I am, waiting for the opportunity of being of use to the College which I hope will be established here.

Had this proposal been made to me before the removal of my library and apparatus hither, the case would have been different; but this being now done, at a great risk and expence, I am, at all events, fixed for the remainder of my life.

If I had come, Mr. Henry would have assisted me in collecting materials, and making the preparations for the necessary experiments. As he is well qualified for the office, if you be not better provided, what should you think of him? At least, I think the students might, with advantage, attend his lectures, till the vacant professorship be filled. This I observe in confidence, wishing the hint to go no further, if you disapprove of it.

With the greatest gratitude and respect, I am,

Dear Sir, yours sincerely,

J. PRIESTLEY.

DOCTOR RUSH, PHILADELPHIA.

The minute of the Trustees of the University of Pennsylvania is also confirmatory of the invitation tendered Dr. Priestley and indicates his final action:

November 11, 1794.

The Board, according to order, proceeded to the election of a professor of Chemistry, in the room of Dr. John Carson, Deceased, when the ballots being taken and counted, it appeared that Dr. Joseph Priestley was unanimously elected.

March 3, 1795.

Mr. Chief Justice (McKean) informed the Board that Dr. Joseph Priestley had declined the Professorship of Chemistry, to which he was elected in this institution the 11 of November last.

Not only Priestley's public papers, written in this country, but also his private correspondence with men of science prove his complete devotion to the cause of "phlogiston," which he was upholding. A letter addressed to Dr. Mitchill, which appears now for the first time in print, makes evident the scientist's ardor for this cause:

Dear Sir,—

I am very glad that your Medical Repository has been extended to subjects of general philosophy and chemistry. Had I known this before, I should have taken the liberty to send you an account of some of my late experiments, especially those which have for their object the decision of the question between the Phlogistians and the Antiphlogistians. I have not yet seen any part of the work, but shall not fail to procure it the first opportunity and consider the history which you say it contains of the controversy between me and my opponents. In the mean time I beg you would communicate to the publishers the following account of an experiment which I wish Dr. Maclean and other advocates of the new theory to consider, and endeavour to explain on their principles. Our common object is the investigation of *truth*, and surely a question of this nature, purely philosophical, may be discussed in the most amicable manner. The pamphlet in which I replied to Dr. Maclean and Mr. Adet I presume he will answer in a separate pamphlet; and having waited for it some time, I am now in dayly expectation of it.

An argument on which, in my late publication, I laid

Second letter from Dr Priestley to Dr Mitchill professor of Chemistry at New York

Dear Sir

I am very glad that your Medical Repository is extended to subjects of general philosophy and chemistry. Had I known this before, I should have taken the liberty to send you an account of some of my late experiments, especially those which bear for their object the decision of the question between the Phlogistians and the Antiphlogistians. I have not yet seen any part of the work, but shall not fail to procure it the first opportunity, and consider the history which you say it contains of the controversy between me and my opponents. In the mean time I beg you would communicate to the publishers the following account of an experiment which I wish Dr Maclean, and other advocates of the new theory, to consider, and endeavour to explain on their principles. Our common object is the investigation of truth, and surely a question of this nature, purely philosophical, may be discussed in the most amicable manner. The pamphlet which I address replied to Dr Maclean and Mr Adet I presume he will answer in a separate pamphlet; and having wanted for it some time, I am now in daily expectation of it.

An argument on which, in my late publication, I laid some stress, is that when inflammable air is procured by the solution of iron in diluted acid of vitriol, there is no addition of oxygen found in the vessel in which the process is made, which ought to be the case if the inflammable air came from the decomposition of the water; and that finery cinder, called by the Antiphlogistians, black oxide of iron, cannot be proved to contain any oxygen at all, tho, according to their principles, it constitutes about one third of its weight. I have since this made a similar experiment with zinc, which is another metal by means of which inflammable air is easily procured, and which I think rather more decisive in favour of my hypothesis, which is that the inflammable air comes from the metal, and not from the water in which it is dissolved; and therefore that metals are compound substances, consisting of phlogiston and peculiar earths, and that water is not decomposed.

On throwing the focus of a burning lens on a quantity of zinc in common air, confined by water, in a glass vessel, the first effect is the production of flowers of zinc, which makes a beautiful appearance, by their dispersion within the vessel; and during this part of the process the air is demonstrated, the pure part of it, no doubt, entering the calx, while the phlogisticated part remains unaffected. After this, the application of the

heat being continued, there is an increase of the quantity of air by the production of inflammable air; and instead of flowers of zinc, a black powder arises, and adheres to the inside of the vessel, and with care may be collected.

Now, since inflammable air is produced, the Antiphlogistians must say that part of the water over which the experiment was made was decomposed. But then I ask where is the oxygen which, according to them, constitutes the far greater part of the water? I cannot find it anywhere. The water is entirely free from acidity, and the air expelled from it afterwards is even less pure than that which it yields before the process. And if I examine the black powder by heating it in confined common air, it becomes a whitish substance, the air is diminished, and rendered in a considerable degree impure; whereas, if it had contained any oxygen, the quantity would have been increased, and it would have been purer than common air; as when red precipitate, or minium, is treated in the same manner. It is evident, therefore, that it contained no oxygen, but a quantity of phlogiston; on the expulsion of which, and the imbibing of pure air, it became flowers of zinc.

This experiment is rather more decisive than that similar one with iron, because the black powder to which zinc is reduced can be affected by heat in common air, which finery cinder cannot.

I have been in expectation of hearing from Mr Bauer Hold, and the other chemists in France to whom any first publication on this subject was addressed; but as there is now no communication between this country and that, I shall be glad to proceed in the discussion of the question with Mr Mallean and other chemists on this continent. I shall attend with candour to anything that they shall suggest, and freely acknowledge any mistake or oversights into which I may have been betrayed; but I hope it will not be taken for granted, that where the results of experiments are differently reported by the French chemists and myself, they are always in the right. ~~and that~~ An impartial judge will see with his own eyes, and if he have not the means of doing this, he should not decide at all.

I am, with great respect,

Dear Sir,

Your sincerely

J Priestley

Northumberland June 14 1790

Dear Sir

I shall be much obliged to you if you will get the preceding letter inserted in the next No of your Medical Repository and 25 Copies struck off, and sent to Mr Dobson for the use of my friends, forwarding one to Benjn Vaughan Esq at Charles Vaughan's Esq Boston, and another to Dr Mordecai.

I am much obliged to you for all the articles you sent me, and shall give them an attentive perusal. That which relates to infectious diseases seems very interesting.

Your attempt to reconcile the two theories was plausible, and well meant, but I do not think they can be reconciled.

Please to return my compliments to Chancellor Livingston. I shall be glad to hear the result of his experiments in gypsum. I am watering some clover with a solution of gypsum. When I see the effect, compared with the common method of using it, I will inform you. I suspect that it acts as a stimulus only. I once thought it might act by attracting moisture. If so, pounded glass may be useful. I shall try that, and other things.

I beg to hear from you oftener, and send. Dear Sir

yours sincerely

J Priestley

Wmt Jun 4th 1798

some stress, is that when inflammable air is procured by the solution of iron in diluted acid of vitriol, there is no addition of oxygen found in the vessel in which the process is made, which ought to be the case if the inflammable air came from the decomposition of the water; and that *finery cinder,* called by the Antiphlogistians, *black oxide of iron,* cannot be proved to contain any oxygen at all, tho, according to their principles, it constitutes about one third of its weight. I have since this made a similar experiment with *zinc,* which is another metal by means of which inflammable air is easily procured, and which I think rather more decisive in favour of my hypothesis, which is that the inflammable air comes from the *metal,* and not from the *water* in which it is dissolved; and therefore that metals are compound substances, consisting of phlogiston and peculiar earths, and that water is not decomposed.

On throwing the focus of a burning lens on a quantity of lime in common air, confined by water, in a glass vessel, the first effect is the production of flowers of zinc, which makes a beautiful appearance, by their dispersion within the vessel; and during this part of the process the air is diminished, the pure part of it, no doubt, entering the calx, while the phlogisticated part remains unaffected. After this the application of the heat being continued, there is an increase of the quantity of air by the production of inflammable air; and instead of flower of zinc, a *black powder* arises, and adheres to the inside of the vessel, and with care may be collected.

Now, since inflammable air is produced, the Antiphlogistians must say that part of the water over which the experiment was made was decomposed. But then I ask where is the oxygen which, according to them, constitutes the far greater part of the water? I cannot find it anywhere. The water is entirely free from acidity, and the air expelled from it afterwards is ever less pure than that

PRIESTLEY'S HOME AND LABORATORY

which it yields before the process and if I examine the *black powder* by heating it in confined common air, it becomes a whitish substance, the air is diminished, and rendered in a considerable degree impure; whereas, if it had contained any oxygen, the quantity would have been increased, and it would have been purer than common air; as when *red precipitate,* or *minium,* is treated in the same manner. It is evident, therefore, that it contained no oxygen, but a quantity of phlogiston, on the expulsion of which, and the imbibing of pure air, it became flower of zinc.

This experiment is rather more decisive than the similar one with iron, because the black powder to which zinc is reduced can be affected by heat in common air, which finery cinder cannot.

I have been in expectation of hearing from Mr. Berthollet, and the other chemists in France to whom my first publication on this subject was addressed; but as there is now no communication between this country and that, I shall be glad to proceed in the discussion of the question with Dr. Maclean and other chemists on this continent. I shall attend with candour to anything that they shall suggest, and freely acknowledge any mistakes or oversights into which I may have been betrayed, but I hope it will not be taken for granted, that where the results of experiments are differently reported by the French chemists and myself, they are always in the right. An impartial judge will see with his own eyes, and if he have not the means of doing this, he should not decide at all.

I am, with great respect,
Dear Sir,
Yours sincerely,
J. Priestley.

Northumberland, June 14, 1798.

DEAR SIR,—

I shall be much obliged to you if you will get the preceding letter inserted in the next number of your Medical Repository, and 25 copies struck off, and sent to Mr. Dobson for the use of my friends, forwarding one to *Benjamin Vaughan, Esq.*, at *Charles Vaughan's, Esq., Boston,* and another to *Dr. Maclean.*

I am much obliged to you for all the articles you sent in, and shall give them an attentive perusal. That which relates to the infectious diseases seems very interesting.

Your attempt to reconcile the two theories was plausible and well meant, but I do not think they *can* be *reconciled.*

Please to return my compliments to Chancellor
I shall be glad to hear the result of his gypsum.
I am watering some clover with a solution of gypsum. When I see the effect, compared with the common method of using it, I will inform you. I expect that it acts as a *stimulus* only. I even thought it might act by attracting moisture. If so, powdered glass may be useful. I shall try that, and other things.

I beg to hear from you oftener, and am,

Dear Sir,
Yours sincerely,
J. PRIESTLEY.

An interesting description of Priestley is given by Silliman in his diary:

This celebrated gentleman was also a guest on one of the occasions, when I dined at Dr. Wistar's. As a very young man (of twenty-three or twenty-four), I felt it an honour and advantage to be introduced to so celebrated an author and philosopher. In 1794 he fled from persecution, and took refuge with his family at Northumberland, Pennsylvania, on the Susquehanna River. Here he resumed his philosophical pursuits, and made occasional

visits to Philadelphia. It was on one of these occasions that I was invited to meet him at Dr. Wistar's table, and the interview was to me very gratifying. In person he was small and slender, and in general outline of person not unlike the late President Stiles (Yale). His age was then about seventy. His dress was clerical and perfectly plain. His manners were mild, modest, and conciliatory; so that, although in controversy a sturdy combatant, he always won kind regard and favour in his personal intercourse. At the dinner, Dr. Priestley was, of course, the honoured guest, and there was no other except one gentleman and myself.

Some of Dr. Priestley's remarks I remember. Speaking of his chemical discoveries, which were very numerous, he said,—"When I had made a discovery, I did not wait to perfect it by a more elaborate research, but at once threw it out to the world, that I might establish my claim before I was anticipated." He remarked upon those passages in the Epistle of John which relate to the Trinity, that they were modern interpolations, not being found in the most ancient manuscripts.* He spoke much of Newton and his discoveries, and the beauty and simplicity of his character; and I think that he claimed him as thinking in religion as he himself did. He mentioned being present at a dinner in Paris given by the Count de Vergennes during the American Revolution, and the seat next to him was occupied by a French nobleman. At another part of the table were two gentlemen dressed in canonicals. When, said Dr. Priestley, I inquired of the nobleman the names of these two gentlemen, he replied: "One of them is Bishop So-and-so, and the other Bishop So-and-so; but they are clever fellows; and, although they are bishops, they don't believe anything more of this mummery of Christianity than you or I do." "Speak for yourself, sir," I replied; "for, although I am accounted a heretic

* Dr. Priestley doubtless referred to I John V, 7.

in England, I do believe what you call *this mummery of Christianity.*" Dr. Priestley, whom I saw on various occasions, when invited to dine, accepted the invitation, but took out his memorandum-book, and noted the engagement, remarking that he had now only an artificial memory. After rejecting the doctrine of Phlogiston in early years, he resumed it at a later period of life; and it was reported at Philadelphia that he was occupied on his death-bed in correcting the proof of a new pamphlet on that subject. He died from inanition, being unable to take any food,—his digestive powers being gone.

Priestley's fund of knowledge was all but boundless; and, in the communication and diffusion of it, he was bounteous to profusion. Though, in neither public or private discourse did he manifest a trait of what is called eloquence, or elegance of style or manner; yet he was one of the most instructive and interesting preachers and colloquists.

So rich was the doctor in valuable colloquial matter, and so bounteously and dexterously did he impart it, that I never passed half an hour in conversation with him that did not add something to my stock of useful knowledge.

Added to his other amiable and attractive attributes, Dr. Priestley was one of the most single-minded and modest of men. For the vast store of knowledge he possessed, he took to himself no credit, except on the score of labor and industry.

Notwithstanding the charge of damning theological heresies that were piled mountain-high against Dr. Priestley, I witnessed in him, on a certain occasion, manifestations of mind and feeling which utterly nullified and scattered them to the wind. I attended him, in consultation with Dr. Rush, in a severe and very threatening fit of sickness, when I greatly feared, and he himself confidently believed, that he was on his death-bed. And never

did I behold any individual, in a like case, more calm and submissive than he was, under present suffering, or more firm and confiding, peacefully resigned and cheerfully hopeful in relation to his condition in a future state. (Chas. Caldwell, M. D. Autobiography, 1855.)

The death of Priestley cast a gloom over all scientific activity in the United States. In the "Medical Repository," [2] V. I, the notice of his death was printed and a last tribute was paid him:

"On the morning of Monday, February 6, 1804, this venerable man (Priestley) paid the debt of nature, and was buried on the Thursday following at Northumberland, in Pennsylvania, where he had lived chiefly since his arrival from Britain. He had been affected, as Dr. John S. Mitchell, of Sunbury, observes, with a stricture at the upper orifice of his stomach for some length of time, which rendered it impracticable for him to swallow any solid food. About two months before his death an inflammation of his stomach supervened, which had the effect of relieving the stricture, by discharging, at intervals, a large quantity of slimy matter. A little after this, oedematous swellings took place in his feet and legs; general debility came on; and he gradually became weaker and weaker, until death closed the scene."

Mr. Samuel H. Smith, editor of the *National Intelligencer,* published at the city of Washington, announced this affecting event in the following respectful terms.

"We have imposed upon us the painful duty of announcing the mournful intelligence of the death of Joseph Priestley, the favourite of science, the advocate of civil and religious liberty, the ornament of the land in which he lived, and the pride of the age from which he received, and on

which he reflected glory. As in the life of such a man the world was interested, so nothing short of the tributary regrets of an universe can duly commemorate such departed greatness. For one, the editor of this paper challenges from those who occupy the sphere of its circulation the solemn admiration merited by him whose career of terrestrial glory has drawn to a close without having been tarnished by the minutest reproach. It were vain to attempt, in this necessarily concise notice, to delineate a character so fertile of intellectual powers as that of Priestley. Few of the subjects in which mankind are the most interested escaped the pen of a writer, the philanthropy of whose heart never slept. There was scarcely a department of natural science not improved or enlightened by his research, and the creative power of his genius; and politics and theology, in their widest range, seemed almost too limited for faculties at once patient and profound. His associates in science will seize the occasion to manifest that whatever insensibility to merit may sometimes unfortunately attach to the political world, the warmest gratitude invariably embalms the memory of those who have eminently distinguished themselves in the walks of philosophy. To the American Philosophical Society, whose annals are brightened by his labours, we look for the memorial of his greatness.''

At a special meeting of the American Philosophical Society, held at their hall the 24th February, Benjamin S. Barton, M. D. was duly elected to deliver an eulogium on the Rev. Dr. Joseph Priestley.

'' 'His principal occupation through life,' says one of his friends, 'was to propagate the evidences of the truth of Christianity, and the belief of the one true God, as revealed by the divine mission of Jesus Christ.

'' 'As a metaphysician, he stands foremost among those who have attempted the investigation of the abstruse controversies in this department of literature. The question

of liberty and necessity, imperfectly understood by the ancients, and on which Bradwardine first threw a ray of scholastic light, was hardly understood by Hobbes, and Leibnitz, and Zanchius, and Jackson, and Clarke. Priestley was the first man who introduced into notice the immortal Hartley, and reduced the question itself within the comprehension of common understandings. When to his publications on this subject are added his disquisitions on matter and spirit, he ranks, beyond controversy, as the first metaphysician of the present age.

" 'As a politician, he has assiduously and successfully laboured, not merely to prepare the minds of his former countrymen of Great Britain to adopt those gradual and salutary reforms in their own system of government, which the democratic part of it so obviously requires, but to extend and illustrate those general principles of civil liberty which are happily the foundation of the constitution of his adopted country.

" 'His profound attention to the belles lettres, and to the other departments of general literature, has been successfully exemplified among his other writings, by his lectures on oratory and criticism, and on general history and policy.

" 'Of the most important and fashionable study of Pneumatic Chemistry, he may fairly be said to be the father. His discoveries of the various gases, which his writings first announced to the world, exceed not merely in number, but in importance, even those of the illustrious Scheele, of Sweden, and the French Lavoisier.

" 'He has contributed to make the present generation of readers think and investigate beyond any writer of his day. His life is closed. He has lived and died an example of the sublime simplicity of character, which has never been attendant but on the first-rate abilities, uniformly exerted for the benefit of mankind.'

"Since his illness in Philadelphia, in the year 1801, he

never regained his former good state of health. His complaint was constant indigestion, and a difficulty of swallowing food of any kind. But, during this period of general debility he was busily employed in printing his Church History, and the first volume of his Notes on the Scriptures, and in making new and original experiments. During this period, likewise, he wrote his pamphlet of Jesus and Socrates compared, and reprinted his Essay on Phlogiston.

"From about the beginning of November, 1803, to the middle of January, 1804, his complaint grew more serious; yet, by judicious medical treatment, and strict attention to diet, he, after some time, seemed, if not gaining strength, at least not getting worse; and his friends fondly hoped that his health would continue to improve as the season advanced. He, however, considered his life as very precarious. Even at this time, besides his miscellaneous reading, which was at all times extensive, he read through all the works quoted in his comparison of the different systems of the Grecian philosophers with Christianity; composed that work, and transcribed the whole of it, in less than three months; so that he has left it ready for the press. During this period he composed, in one day, his second Reply to Dr. Linn.

"In the last fortnight of January his fits of indigestion became more alarming, his legs swelled, and his weakness increased. Within two days of his death he became so weak that he could walk but a little way, and that with great difficulty; for some time he found himself unable to speak; but on recovering a little, he told his friends that he had never felt more pleasantly during his whole life-time, than during the time he was unable to speak. He was fully sensible that he had not long to live, yet talked with cheerfulness to all who called on him. In the course of the day he expressed his thankfulness at being permitted to die quietly in his family, without pain, and

with every convenience and comfort that he could wish for. He dwelt upon the peculiarly happy situation in which it had pleased the Divine Being to place him in life, and the great advantage he had enjoyed in the acquaintance and friendship of some of the best and wisest of men in the age in which he lived, and the satisfaction he derived from having led an useful as well as happy life. He this day gave directions about printing the remainder of his Notes on Scripture (a work in the completion of which he was much interested), and looked over the first sheet of the third volume, after it was corrected by those who were to attend to its completion, and expressed his satisfaction at the manner of its being executed.

"On Sunday, the 5th, he was much weaker, but sat up in an arm chair for a few minutes. He desired that John XI. might be read to him. He stopped the reader at the 45th verse, dwelt for some time on the advantage he had derived from reading the scriptures daily, and recommended this practice, saying, that it would prove a source of the purest pleasure. 'We shall all', said he, 'meet finally: we only require different degrees of discipline, suited to our different tempers, to prepare us for final happiness.' Mr. ——— coming into his room, he said, 'You see, Sir, I am still living.' Mr. ——— observed that he would always live. 'Yes, I believe I shall; we shall meet again in another and better world.' He said this with great animation, laying hold of Mr. ———'s hand in both of his own. After evening prayers, when his grand-children were brought to his bed-side, he spoke to them separately, and exhorted them to continue to love each other, &c. 'I am going,' added he, 'to sleep as well as you; for death is only a good long sound sleep in the grave; and we shall meet again.'

"On Monday morning, the 6th of February, on being asked how he did, he answered, in a faint voice, that he had no pain; but appeared fainting away gradually. About eight o'clock he desired to have three pamphlets, which

had been looked out by his directions the evening before. He then dictated, as clearly and distinctly as he had ever done in his life, the additions and alterations which he wished to have made in each. Mr. ——— took down the substance of what he said, which was read to him. He observed, 'Sir, you have put it in your own language; I wish it to be in mine.' He then repeated over again, nearly word for word, what he had before said; and when it was transcribed, and read over to him, he said, 'That is right; I have now done.'

"About half an hour after he desired that he might be removed to a cot. About ten minutes after he was removed to it he died; but breathed his last so easily, that those who were sitting close to him did not immediately perceive it. He had put his hand to his face, which prevented them from observing it.

"He was born March 24, 1733.

"Perhaps no man was ever more conscious of the approach of death than Dr. Priestley, or made more exact arrangements for that solemn event. In one of his letters to Dr. Mitchill, dated January 9, 1802, he expressed himself thus:—'I am at present very much behind-hand in philosophical intelligence, by which I suffer much. In winter also I am not fond of going much into my laboratory, so that I do very little in the way of experiments at present, though in other respects I am not quite idle. I feel, however, the effect of years, and I am by no means so active as I have been. Neither have I recovered from the effects of the fever that I had in Philadelphia. I am much weaker and thinner, and this, I fancy, has in some measure been the cause of the ague I have had lately, and which I never had before.'

"His attachment to the administration under Mr. Jefferson was strong and ardent. In another letter to Dr. Mitchill, of January 8, 1803, he has this paragraph:—'I think myself much honoured by the respectful mention of

me by your friends in Congress, and could wish to pay them a visit; but at my time of life, the inconvenience of a journey at this season of the year would be too great for me. As to the chaplainship to Congress, I should not think of it. They have my best wishes, and prayers too, without any salary. I rejoice in the present aspect of public affairs, and hope it will be long continued. Our excellent President will, I doubt not, put war and every other evil as far as he can from us.'

"On the 25th January, a few days before his death, he wrote the following to Dr. Logan:—'By means of various illnesses I am reduced to a state of extreme debility; and if the swelling that began at my feet, which has now reached my knees, should continue to advance as it has done, my continuance here cannot be long. But I have lived a little beyond the usual term of human life, and am content and thankful. Few persons, I believe, have enjoyed life more than I have done.

"Tell Mr. Jefferson that I think myself happy to have lived so long under his excellent administration, and that I have a prospect of dying in it. It is, I am confident, the best on the face of the earth, and yet, I hope, to rise to something more excellent still.'

"To those who are desirous of tracing the scientific progress of Dr. Priestley, since his arrival in America, it may be matter of pleasing information to learn, that a very large part of his publications on these subjects are contained either in their original forms, or in review, in the first Hexade of the Medical Repository."

CHAPTER VI

THOMAS COOPER (1759-1841), a graduate of University College, Oxford, and a gentleman of wide learning, followed Priestley to this country. He had been attracted to the scientist in England and was not in sympathy with the oppression that was visited upon Priestley.

Cooper was elected to the professorship of chemistry and mineralogy in Dickinson College, Pa. Very definite religious views, opposed to so-called free thought, were entertained by that institution. A number of the Trustees hesitated to vote for Cooper, but Benjamin Rush, one of them, insisted upon his election. He spent three years as professor of chemistry and mineralogy at Dickinson College. While there he gave to the College the glass used, by the discoverer of oxygen, to focus the sun's rays on "red precipitate," and other apparatus originally owned by Joseph Priestley.

He came to Philadelphia in 1815; and the following year was elected to the Chair of Chemistry and Mineralogy in the University of Pennsylvania, where he taught for four years.

In 1811, he published a paper entitled "An Account of the Decomposition of Potash and the Production of Potassium by Heat." He carried out the work in Priestley's Laboratory at Northumberland.

Thomas Cooper

It is probably the first time that potassium was made in this country. A letter sent by Cooper to his son-in-law, in which his experiments were described, is interesting and well deserves a place among these early writings:

DISCOVERY OF A NEW METAL, POTASSIUM.

MR. EDITOR:

Having just received from my father-in-law, Judge Cooper, of Northumberland (unanimously chosen a short time ago, Chemical Professor at the College of Carlisle), an account of the first successful attempt at making the new metal POTASSIUM in this country, I send it to you; believing many of your readers will feel interested in the detail of an experiment so beautiful, as well as so important to the theory of Chemistry,

I am, sir, your obedient servant,

J. MANNERS.

Extract of a letter from Judge Cooper of Northumberland to Dr. Manners of this city (Philadelphia).

NORTHUMBERLAND, June 28, 1811.

DEAR SIR:

About a fortnight ago, Mr. Reuben Haines of Philadelphia, brought me a few small pieces of POTASSIUM, which Mr. W. Hembell was so kind as to send me; he procured them from Mr. Johns, who had repeatedly made it, as I understand, at Mr. Davy's laboratory at the Royal Institute in London, from whence he brought some to Philadelphia. Mr. Johns, in company with Dr. Coxe, attempted to make it at Dr. Coxe's laboratory, but owing to some accident, the experiment did not succeed: probably Dr. Coxe has succeeded ere this; as a failure in the first repetition of a chemical experiment, is too common to furnish any ground of discouragement. The phenomena afforded by this substance were so pleasing and so extraordinary that

I was tempted to take a few days from my translation of JUSTINIAN, and devote them to the making of POTASSIUM. On perusing the account given of the method of making potassium at the Royal Institute, in 25 Nich. Jour. 191, and Mr. John's variation of the Apparatus in 35 Tilloch's Phil. Mag. 321, I preferred the latter. See the plate. Having picked out a gun barrel from Dr. Priestley's laboratory, I gave it to an ingenious workman here (Jas. Macklay) to cut and bend. The barrel was not thick, and he tried to give the required curve by filling it with melted lead, but that did not succeed. It was bent by gradual heating and hammering. In bending, it cracked.

I took another, which was also bent according to the drawing I send you. The piece cut off was accurately filed and ground with emery and pumice, to fit the sloping end of the curved piece which projected beyond the furnace. Not being able in this little town to find any clean iron filings or turnings, I made the man patiently chip some soft iron in small pieces sufficient to fill the curvature of the gun barrel. The straight piece (or alonge) with the brass cock and tube was adjusted, the joint luted, the curvature raised to a white heat, and the breeching end of the gun barrel, which also projected out of the furnace about five inches, was made red hot with coals surrounding it, supported by a piece of sheet iron. About an ounce of the causticum commune fortius, very carefully prepared by myself for the purpose, was inserted at the larger end of the barrel, the screw of the breeching was then put in and luted, and the end of the glass tube inserted in a basin of oil. The heat was kept up for about half an hour. The apparatus left to cool; when opened, the alkali was found distilled over unchanged, and not the slightest appearances of potassium. On examining the gun barrel, three small holes were found, either burnt by the fire, or occasioned by some imperfection in the gun barrel itself.

I took another gun barrel and treated it the same way,

but on examining after the curvature was made, it was found also to have some flaws.

I procured the gun smith to pick me out a thick heavy barrel not yet bored for a rifle. It was bent, cut, and treated like the others. The curved part within the furnace was filled with iron cuttings and turnings. The strait piece or alonge, was ground to fit the end of the curved piece: but during the operation it was found not to be quite tight: yet as it was well luted, the experiment was allowed to proceed. The same process was begun and continued as before: the apparatus cooled; taken to pieces; examined; the alkali was distilled over, but no potassium! In all the cases much hydrogen gas escaped at the end of the tube immersed in the oil, but at no period of the operation was there any absorption; which convinced me, the apparatus was not tight enough in its separate parts.

I had the alonge again ground more accurately, and though Mr. Johns says this is enough if air-tight, which mine always was, I had it when well adjusted by means of grinding, still firmer fixed by three small screws, which I take to be a necessary precaution. The curved part was filled (instead of iron cuttings) with a faggot of small clean iron wire as thick as we could introduce; the apparatus was refitted; I made with great care a fresh portion of caustic alkali, and the process recommenced. On cooling and examining the apparatus, the alkali had distilled over, and on the external surface of it, slight but unequivocal signs of potassium appeared.

I again repeated the experiment next day (June 24), substituting iron wire, clipped into pieces of about the eighth of an inch long, of which about half a pound was necessary to fill the curve of the barrel within the furnace. The white heat was carefully given to the barrel within the furnace, which was again filled up with charcoal. The breeching end on the outside was made red hot; the alkali, very dry, was inserted at twice, and two or three minutes

of interval allowed for the hydrogen gas, arising from the decomposed moisture, to burn away. The whole being put in, the screw of the breeching was luted with fat lute (lime and boiled oil); so was the joint where the alonge was screwed into the end of the curved barrel, and the joint where the brass cock was inserted into the alonge, and the place where the glass tube was inserted into the brass cock. The heat was kept up strongly for a full half hour. On cooling the apparatus, potassium, in its most perfect state, was found within and without side the small internal tube, and in the end of the gun barrel next the wire.

I obtained about as much as filled at least one fourth of an ounce vial. Some of it took fire in the air, and some I used before I had collected the whole.

It was of the colour and lustre of pure silver on the outside: on being cut it was equally metallic, but rather more blue and mercurial internally. It was soft and could be cut and spread very easily.

It decomposed water with a very vivid pink-coloured flame, moving on the surface with a rapid irregular gyratory motion.

I had not sufficient of naphtha, to cover the one half of it: therefore I put it first into olive oil: in this it sank with some difficulty: streams of air issued from it, and occasioned a strong frothing in the oil; wherein it was gradually decomposed.

It sank in spirits of turpentine. A stream of gas issued from each piece; the liquid was turned of a brown colour and become opaque; the metal in about three hours was decomposed, leaving the same kind of thin metallic grains which are seen when it is decomposed in water, and which, for the present, I agree with Mr. Haines in supposing to be the particles of iron combined with the metalline potassium. The spirits of turpentine are partially converted into a soap, forming an opaque milky mixture with common water. I put the larger portion in the vial sent me by Mr.

Hembell, which contained a small quantity of naphtha, whether enough to preserve the metal tolerably pure till he receives it in the city, I know not.

Mr. Reuben Haines was so good as to assist me throughout these experiments. I have been thus minute in detailing my want of success, because such a detail may serve to put others on their guard, and save time and expense. For the same reason, I shall state the precautions I took in making the caustic alkali, which I had to prepare three times for this operation. Perfectly pure alkali I find sells in London for eight times the price of the common caustic, but whether it be made by repeated crystallizations of the carbonat, by Lowitz's or by solution in alcohol after Berthollet's method, I know not. The latter is certainly not pure alkali, as it has been ascertained that it will not saturate an equal quantity of acid, with good caustic alkali well prepared in the common way.

I took one and a half or two pounds of lime burnt in the common way; I burnt it over again in a black lead crucible in a strong red heat for four hours: I weighed equal weights of this lime while hot, and clean white pearl ash bruised tolerably fine, I mixed them together and poured about one and a half gallon of boiling water on two pounds of the mixture. I stirred it well, covered it, and let it stand for two hours. I then tried the liquor, which shewed no signs of effervescence, with an acid. The supernatant solution being poured off, as much more boiling water was added. The solution filtered and evaporated in a brass basin (which I had previously silvered, though I do not know it was absolutely necessary) was still further gradually evaporated in a clean brazed sheet iron crucible, and then heated to a low red heat, when the alkali became fused without any signs of ebullition. In this state it was poured out broken into small pieces quickly and bottled while hot. During evaporation it attracts carbonic acid from the atmosphere, and becomes very slightly effervescent. It should

be evaporated if possible in a sand bath over a close fire. In this case it is absolutely necessary the apparatus should be set in such a place that the fumes of the charcoal may be carried off. I spoilt one preparation by not sufficiently attending to this circumstance, and was compelled to treat the solution with a fresh portion of lime. I enter into this detail, because we have no London or Paris in America. I doubt whether the common alkaline caustic kept in the shops is sufficiently good and pure, and an experimenter here must depend much upon his own resources. The alkali thus fused in a low red heat still contains, as I think, near 20 per cent. of water; for the great quantity of hydrogen gas extricated after putting in the alkali can only proceed from the decomposition of the moisture in the caustic potash, by means of iron.

This metal was first procured by professor Davy, in the progress of his most interesting experiments with the Galvanic battery. The method of procuring it by means of iron we owe, I believe, to Thénard and Gay-Lussac (1808). Our Dr. Woodhouse, before his death (1809), certainly decomposed caustic potash by fusing it in a closed crucible with lamp black, and procured a substance that took fire in the air. Curadeau in 66 "Ann. de Chimie" 97 (37 Phil. Mag. 283) proposes the same kind of process without once noticing the experiment of Dr. Woodhouse.*

The following are the different kinds of apparatus yet contrived for distilling pure potash over iron.

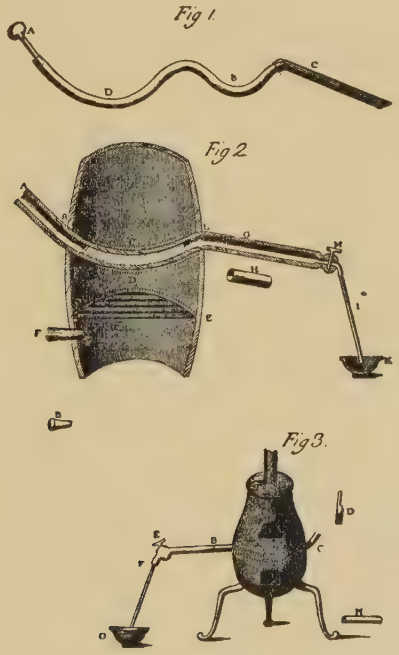
Fig. 1. The apparatus used at the Royal Institute. 25 Nich. Jour. 191.

A common gun barrel, very clean inside, has an iron receptacle A ground into one end of it and furnished with a ground stopper, capable of holding 2 or 3 ounces of fused potash. Clean iron turnings are placed in the curve at D, and brought to a white heat. The potassium distils over

* He evidently did this before Gay-Lussac and Thénard published their method, and deserves recognition for his effort.

at B, which is kept cool. Common air is excluded by a glass tube C, which supports a column of mercury. The tube is cut to get at the potassium.

Fig. 2. Mr. Johns's apparatus, by which the tube is saved, and the experiment may be repeated without destroying any part of the apparatus. 35. Phil. Mag. 321.



An iron gun barrel is cut in three pieces: the first, A a; the second forms the curve a L; the other is the strait piece or alonge G. This is fitted to L, by accurately grinding the one end to fit into the projection of the curved barrel. M is a stop cock, I a glass tube, K a small basin containing olive oil. H is a tube of sheet iron, about six inches long, one half of which is inserted in the curved part L, and the other in the strait part of the barrel G, before they are

fitted to each other. The potassium distils into this, and is more easily collected than if the barrels alone were used. The furnace is an eight inch black lead crucible, urged with a bellows. Of which the nozzle is shewn at F. B is the stopper to A, which is the receptacle of the pure potash. A is kept cool till D is at a white heat. G is kept cool during the whole operation, to condense the potassium. As soon as absorption appears in the end of the glass tube immersed in oil, turn the cock at M.

Fig. 3. The apparatus which I used, as fixed in a portable air-furnace, nine and a quarter inches internal diameter in the widest part.

A, the furnace, with a hole in the upper part to feed the fire, and a hole below for the ash hole and draught. It consists of two parts, one placed on top of the other where the gun barrel appears outside. It stands on an iron tripod.

B, the place where the strait tube is joined to the curved tube, by accurate grinding the one with the other, then fastening them with three small screws, and luting with fat lute to exclude all chance of moisture.

C, the thick or breeching end of the gun barrel, in which the caustic alkali is put, at twice.

D, the common screw of the breeching of the gun barrel; when all the alkali is in and begins to flow, fix the screw in a hand vice, and put it in its place.

E, the small brass cock, inserted in the strait iron tube by means of a cork, and then luted with fat lute on a strip of linen and tied.

F, the place where the glass tube is inserted into the nose of the brass cock by means of a cork, perforated by a hot iron the size of the tube. Smear it before insertion with a little fat lute, and fix it tight with some tow.

G, a small basin, containing about half a pint or more of olive oil.

H, a brazed sheet iron tube inserted at B, half in one barrel and half in the other; 6 inches long. The strait tube

may be kept cool by a damp cloth repeatedly employed between E and B. It may be supported by a piece of board underneath resting on the ground. Take care no water runs down to the cock or into the basin.

When the absorption begins in the glass tube, after all the hydrogen is expelled, turn the cock and prevent the oil from rising more than half way up the tube.

Upon the whole Johns's is the cheapest apparatus. The labour of bending and grinding and fitting the gun barrel, and the gun barrel itself, is lost each time in Mr. Davy's method, which in this country cannot be an expense each time of much less than one and a half dollars.

Cooper was the editor of Thomas Thomson's "System of Chemistry." He had learned in France the secret of making chlorine from common salt and attempted to become a calico bleacher in Manchester, England, afterward a printer, but he was not successful at either occupation. He was a lawyer before he began his career as a chemist at Carlisle, Pennsylvania.

In 1816, he delivered a lecture on the "Importance of Chemistry to the Medical Man." Cooper had at this time been made Professor of Chemistry in the college department of the University of Pennsylvania, and Robert Hare had been chosen for the chair of Chemistry in the medical department. The faculty of the medical department saw no imperative need of the science of chemistry in a medical student's curriculum; but, if the subject must be taught, the professor did not require a knowledge of medicine. Hare, therefore, was satisfactory to them. He, however, was not to pass the students in his branch. The chemistry examination was to be given by Hare in the presence of two or more of the professors of medicine, and they were

to decide the candidate's mark. Thomas Cooper took exception to this attitude toward the subject of chemistry, and, in the lecture referred to, declared that chemistry was of the utmost importance to medical men, and proceeded to show that all of the secretions of the body are explained by chemical processes. Chemistry was strongly defended by the lecturer, before the faculty and students of the Medical School of the University, and Cooper succeeded in convincing his hearers of their error.

But Cooper was not only a writer on chemical topics—every one recalls the remarkable sentence that Abraham Lincoln used in his speech at Gettysburg, "for the people, by the people," etc.

In the *Outlook* some years ago there appeared the following: "Although much has been written about President Lincoln's Gettysburg address, it may not be amiss, even at this late day, to cite an early authority for the phrase, 'government of the people, by the people, and for the people.' It is found on page 53 of a book bearing the title: 'Some Information Respecting America, Collected by Thomas Cooper, Late of Manchester, London: 1794.' Most of its contents were reproduced in Volume III of 'An Historical, Geographical, Commercial, and Philosophical View of the American United States and of the European Settlements in America and the West Indies,' a bulky but once popular compilation, in four volumes, by W. Winterbotham, published in London in 1795 and sold in the United States."

The extract referred to, entirely aside from its use of this phrase, is not devoid of interest as a description of political and social conditions. It runs as follows:

There is little fault to find with the government of America, either in principle or in practice: we have very few taxes to pay, and those of acknowledged necessity, and moderate in amount: we have no animosities about religion: it is a subject about which no questions are asked: we have few respecting political men or political measures: the present irritation in men's minds in Great Britain, and the discordant state of society on political accounts is not known there. The government is the government *of* the people and *for* the people.

In Cooper's original book, the words "of" and "for" are printed in italics; in the pirated edition, they are in small capitals.

Thomas Cooper had an interesting and varied career and deserves to be recalled as one of our many long-forgotten worthies. He was born in London, in 1759. From 1811 to 1814 he was, as previously remarked, professor of chemistry in Dickinson College, at Carlisle; from 1816 to 1820, he held the same relation to the University of Pennsylvania; and from 1820 to 1834, he was president of the College of South Carolina, attaining distinction as an extreme advocate of the States' Rights doctrine during the nullification period. He died in Columbia, S. C., in 1841.

Remarkable for the extent of his knowledge, he was a materialist in philosophy and a free-thinker in religion. A voluminous writer on law, science, medicine, and political economy,* it is not at all unlikely that his works—current during the first generation of this century—may have come to the notice of Lincoln as a young man; nor would it be

* Lectures on the Elements of Political Economy. By Thomas Cooper, M. D. Second edition, with additions. Columbia, S. C. 1829.

surprising for him to give new currency, in almost its exact form, to a sentiment written seventy years before. If this supposition be correct, time will have brought in one of his revenges by preserving—through the utterance and massive influence of another—a single idea out of many put forth by a man who, beginning his long life as a revolutionist in England, ended it, in a distant clime, as the extreme advocate of States' Rights.

(George F. Parker, *Review of Reviews,* Vol. 23 (1901), p. 196.)

In the autobiography of Charles Caldwell appear the following paragraphs:

In talents, attainments, and general character, Dr. Cooper was one of the most extraordinary men of the day. In literature and science (political science excepted) his views were deep, comprehensive and sound. But, in politics, so thoroughly were his notions infected and perverted by the groundless and wild doctrine of liberty and equality, that his benevolence and humanity alone prevented him from being a Jacobin.

He was by birth and education an Englishman, and, in consideration of his anti-monarchical principles, was elected, during the period of the "Reign of Terror" in Paris, and took his seat, as a member of the National Assembly of France. But his membership in that turbulent and tyrannical body was of short duration. Being of a temper in some degrees fierce and fiery, and a spirit fearless, haughty, and incontrollable, he became engaged in a personal contention with Robespierre, during a sitting of the Assembly, in which the latter used, in relation to him, unbecoming and offensive language. As soon as the session was closed, Cooper determined on satisfaction for the insult, sought the Frenchman, met him in the street, pronounced him a scoundrel (un coquin), drew his sword, and bade him de-

fend himself. Robespierre declined the combat, but prepared for revenge on the daring Englishman. His design was to have him secretly assassinated, or to denounce him in the next meeting of the Jacobin Club, where his influence was irresistible, and have him immediately conducted to the guillotine. Informed of this by a friend, who had in some way penetrated the intention of the French demagogue and convinced that flight alone could save him, Cooper instantly left Paris, and had the good fortune to escape the meditated vengeance.

On his return to England, he found the public mind greatly agitated, and everything in a very perturbed condition, by the actual existence and outrages of mobs in various parts of the kingdom, and the suspicion and reports of plots, insurrections, and concerted rebellion. Nor was this all. He, himself, became suspected to be a leader among the malcontents, the dwelling of his friend Priestley had been assailed by a mob, and all his furniture and fine library burnt; in consequence of which, and the dread perhaps of further violence, the doctor himself was preparing to migrate, or had migrated to the United States.

Influenced by these and probably other considerations, Cooper determined to exile himself from his native country, whose inhabitants, and himself, as one of them, he held to be deeply wronged and oppressed, by a corrupt and tyrannical government, and try his fortune in a foreign land. Under these impressions with regard to political control, and with "liberty and equality" as his battle motto, he selected the United States for his field of future action, and Philadelphia, then our largest and in all respects our chief city, for his place of residence. And from an improvidence as to means, which made a part of his nature, he was low in funds.

Philadelphia was then the seat of the National Government. Congress was in session when Cooper arrived, and Washington, Hamilton, Jefferson, Jay, Madison, Els-

worth, King, and many other distinguished men, statesmen, and politicians were on the spot and in action. And the Goddess of Discord was already among them, and had divided them into the original parties of Federalist and Anti-Federalist—the former being the advocates of a more concentrated and powerful government, administered and directed by legislators and officers appointed for the purpose; and the latter of a government, with a basis as spacious as the populated portion of the Union, of which every man, who wore a head and wagged a tongue, was in part (and that part far from being inconsiderable) a legislator and an executive agent.

At the head of the Federal party was Hamilton—of the Anti-Federal, Jefferson—and their immediate aids, who consisted of the ablest and most influential statesmen and politicians in the country. Washington, too high, patriotic, and pure-minded, to be approached by party spirit, was, as his august title implied, President of the United States.

In this condition of things, strengthened not a little by his own pecuniary condition, Cooper was obliged to look for a subsistence to some public employment connected with the profession of law, to which he had been bred; but which, as far as I remember, he had never yet practiced. And that he might the more readily succeed in procuring some appointment, it was expedient that he should attach himself to one of the political parties. Nor was he long in making his choice. Nature and education appeared to have combined in fitting him for many things—but pre-eminently for three—to be a "liberty and equality" philosopher and projector, a party politician, and a political agitator. Hence, he instinctively attached himself to Jefferson and the *Outs*. True, Jefferson was Secretary of State, and therefore, officially one of the *Ins*. But in principle, wishes, and resolution, he was an *Out;* because his object was to supercede Hamilton, oust Washington, or at least prevent his re-election to the office of Chief Magistrate, and be

promoted to his place. And that promotion he expected from the Anti-Federal party.

By several papers which he wrote, and for which he was probably paid, Cooper was not long in convincing his party of his dexterity and strength in the use of his pen, and therefore of his power to aid them in their projects. And to the employment of it, chiefly, as there is reason to believe, he was indebted for his subsistence for several years. The State of Pennsylvania being then, as it is now, democratic in its government, he was at length appointed to a judgeship in it—but of what court, or with what salary, I do not remember—if, indeed, I was ever informed—for, at that period my acquaintance with the judge was but slight. His tenure of the office, however, did not prove to be either "for life," or until terminated by promotion. On account of some act regarded as official malversation, he was impeached.

But it is certain that the misfortune did not take from him a tittle of his reputation as a powerful, a learned, and a perfectly upright and honorable man. His standing in society, therefore, and his connection and intercourse with the first men and families in the country, were untouched.

Nor was it long until authentic evidence to the effect appeared in his election to the Professorship of Chemistry and Moral Philosophy in Dickinson College, Pennsylvania. In that institution he remained, by far its ablest, and one of its most faithful and popular teachers, until the occurrence of a serious and threatening rebellion, in the quelling of which he manifested, in no common degree, the courage and energy for which he was remarkable. The consequence of the outbreak was a temporary suspension of the exercises of the institution, a slight change in its government and economy, and the resignation of some of its officers—and Cooper, for what reason I know not, never returned to it.

After this, he made Philadelphia his home. In a short

time he was appointed to the Professorship of Chemistry in the University of Pennsylvania; and, later, to the Professorship of Chemistry in Columbia College, in South Carolina, which subsequently led to his elevation to the presidency of that institution.

When Dr. Cooper retired from the position of President of Columbia College, on account of some misunderstanding with the Board of Trustees, he was employed by the Legislature to write a history of South Carolina. Whether he lived to finish that work I am not informed. My impression is, however, that he did not, but died while engaged upon it, at the advanced age of four score and upward—leaving behind him a family but no estate, notwithstanding the labours of his never-idle and protracted life.

Not only was Cooper's mind uncommonly keen and penetrating, it was one of the most inquisitive minds I have ever witnessed. Hence, the field of knowledge it traversed was almost illimitable. It grasped at everything, especially at everything new and curious.

Dr. Cooper was a man of low stature, but robust, well proportioned, and very compactly built, his head was large and finely developed, and uncommonly round, his neck stout and thick, his chest capacious.

Having spoken of Dr. Priestley as a friend of Dr. Cooper, I shall offer a few remarks on that extraordinary man.

It is hardly less than extraordinary that a friendship so strong and fervent as theirs was, should have existed between him and Cooper. For it would be difficult to find two men more dissimilar to each other. The only mutual similarity that marked them was, that each of them possessed talents of an exalted order, and information of great variety and extent. But the character of their intellects, their temperaments, and tempers, and their modes of using their information, were strikingly unlike.

In their scrutinies and discussions of subjects, Cooper's intellect was the more keen, penetrating and searching; Priestley's was the more diffusive, expanded and liberal. Priestley possessed the greater amount of knowledge; Cooper made the most powerful use of what he did possess. In discussion and debate, Priestley was calm, placid and candid; Cooper, vehement, fiery, and sometimes inclined to confuse, perplex and entrap his antagonist. The spirit and manner of the latter resembled those of the advocate resolved, by any admissible means, to succeed in his cause; those of the former the spirit and manner of the judge, summing up the evidence and delivering his charge.

Although Priestley made more discoveries in science than Cooper, yet had he a less original, strong, and philosophical mode of thinking. Hence he depended more on the works of others, and consulted books to a greater extent. He also experimented on a wider scale, and in a more promiscuous and independent manner, and, therefore, made some of his experiments by accident. I mean that he made discoveries other than those which he contemplated; and was so fortunate as to make many when he contemplated none at all. He merely brought substances into contact, or within striking distance of each other, and observed and noted the effect, and thus discovered new and unexpected facts and relations of which he afterward availed himself for useful purposes.

Cooper, on the contrary, had little or nothing of haphazard in his actions. Whatever he did was designed for the attainment of some definite end. And if he failed in that, his failure was regardless of everything else. Hence, in the course of his experiments, or series of experiments, he discovered or picked up nothing accidentally by the way. Nor had he the patience of Priestley to persevere in the repetition of barren experiments, or in the trial of new ones for the same purpose. In a word, he was a neck-

or-nothing man and, therefore, never content with small results.

Cooper was one of the editors, and for a time the sole editor, of "The Emporium of Arts and Science." This was an annual publication in which a review was given of the most interesting discoveries in science. Six volumes of the work are in the possession of the writer. These six do not represent the entire set. They are very rare. Cooper also edited several text-books of Chemistry.

CHAPTER VII

ON page 92 appeared the article in defence of Priestley and against Dr. Maclean (1771-1814) of Princeton. At the close of the 18th century Maclean was regarded as one of the first chemists in the country. His appointment at Princeton dates from Oct. 1, 1795. He was an ardent antiphlogistian. His published contributions deal mainly with "combustion" and the erroneous teachings of the doctrine of phlogiston. They appeared in the *Medical Repository*. He was associated with Silliman in presenting to American students the first foreign edition (1808) of Henry's Chemistry. Silliman has left a delightful remembrance of Maclean:

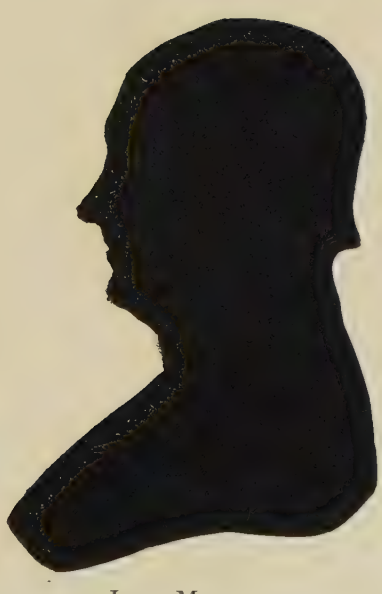
At this celebrated seat of learning (Princeton), an eminent gentleman, Dr. John Maclean, resided as the Professor of Chemistry, &c. I early obtained an introduction to him by correspondence, and he favored me with a list of books for the promotion of my studies. Among these were Chaptal's, Lavoisier's, and Fourcroy's Chemistry, Scheele's Essays, Bergman's Works, Kirwan's Mineralogy, &c. I also passed a few days with Dr. Maclean in my different transits to and from Philadelphia, and obtained from him a general insight into my future occupation; inspected his library and apparatus, and obtained his advice regarding many things. Dr. Maclean was a man of brilliant mind, with all the acumen of his native Scotland; and a sprinkling of wit gave variety to his conversation. I regard him as my earliest master of chemistry, and Princeton as my

first starting point in that pursuit; although I had not an opportunity to attend any lectures there. Mrs. Maclean was a lovely woman, and made my visits at the house very pleasant to me. She was a sister of Commodore Bainbridge, afterwards signalized by the capture of the British frigate Java, in the war of 1812-15. Mrs. Maclean gave me an introduction to the family of Commodore Bainbridge in Philadelphia, in which I was an occasional visitor.

Among the contemporaries of Maclean was James Hutchinson (1752-1793), of the University of Pennsylvania. He was the predecessor of Woodhouse in the chair of chemistry. His training in the science had been received abroad, principally under Fothergill. He must have attained considerable proficiency in his chosen science, because, in 1774, he was the recipient of a gold medal bearing the inscription "for his superior knowledge in chemistry." He was an intense patriot. He organized the medical corps under Washington, and hence probably had little leisure for experimental research.

At Harvard, in 1783, was Aaron Dexter (1750-1829), who, as far as can be learned, never published any research work, and the statement is made in the history of Harvard that he was not a very successful teacher. One thing, however, for which the country at large is indebted to him, is that he was instrumental in having endowed in 1791, the chair of Chemistry in Harvard, known as the Erving Chair of Chemistry.

In 1792, Dr. Samuel Latham Mitchill (1764-1831), was elected to the Chair of Chemistry and Natural History, in Columbia College. He opposed in a very friendly way,



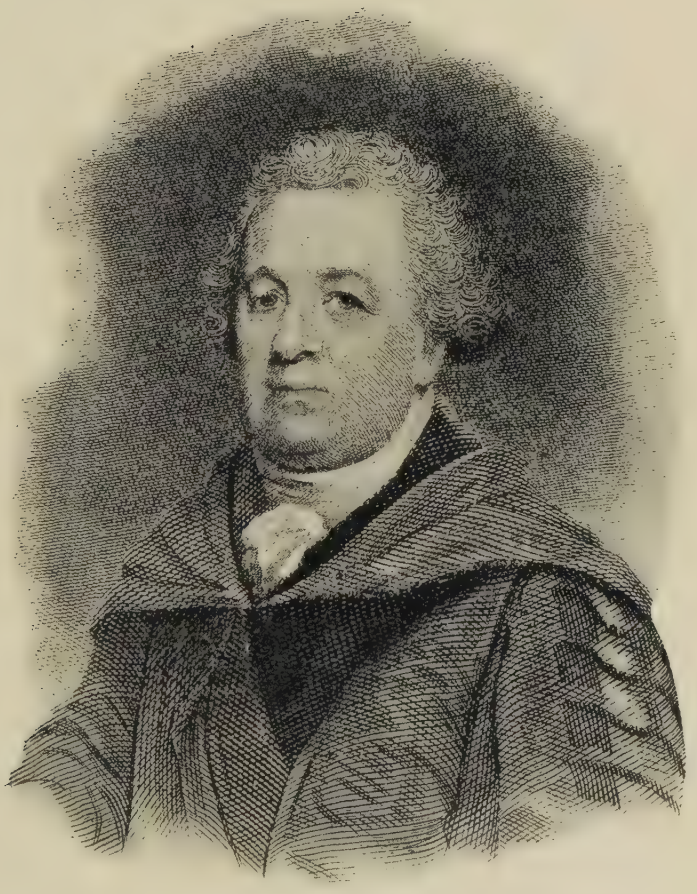
John Maclean
The first professor of chemistry
in the College of New Jersey
from 1795 to 1812

the views of Priestley on Phlogiston; and was the first teacher of chemistry in this country to use the nomenclature of Lavoisier. He was a man of a very strong mind, and very learned. Many lines of investigation received his attention. He founded the *Medical Repository,* the first paper in this country devoted to general science as well as medical science. In 1800 he published a chemical paper "On the non-action of nitric acid on silver, copper, and tin," and, later, "Some interesting particulars on the history of muriate of soda," which appeared in the "Transactions of the American Philosophical Society." Another paper, "Observations on soda, magnesia and lime, in the water of the ocean, and how the water of the ocean may be rendered fit for washing without the aid of soap," also appeared in the "Transactions." In 1804 he published "A Sketch of the Mineralogical History of the State of New York," and, in 1809, a paper bearing the title "Discourse on Mineralogy." Mitchill published a syllabus on the "Synopsis of Chemical Nomenclature and Arrangement." He was probably the first American to write on chemical philosophy.

Duyckinck enumerates one hundred and eighty-nine distinct achievements or important acts of his busy life. He was just forty at this eventful period. His public life embraced six or more years as a member of Congress, and he was in the United States Senate from 1804 to 1809; but he found opportunity meanwhile to be of essential service in innumerable ways to New York. His medical career and contributions to literature, gave him a wide fame; he became in course of years an active member of nearly all the learned societies of the world. He was a sort of human dictionary whose opinion was sought by all originators and

inventors of every grade throughout his entire generation. His analysis of the Saratoga waters greatly enhanced the value and importance of those mineral springs. His ingenious theory of the doctrines of septon and septic acid gave impulse to Sir Humphry Davy's vast discoveries; and his essays on pestilence awakened inquiry all over the world. He was a polished orator, a versifier and a poet, a man of infinite humor and excellent fancy. His eccentricities furnished material for the wits of the day to fashion many a joke at his expense, over which no one laughed more heartily than himself. He was equally at home in studying the geology of Niagara or the anatomy of an egg, in offering suggestions as to the angle of a windmill or the shape of a gridiron, in deciphering a Babylonian brick or investigating bivalves and discoursing on conchology, and in advising how to apply steam to navigation or in disputing about the Bible with his neighbor the Jewish Rabbi. He possessed a charm of manner and a magnetism of mind that was unusual; and he did much to advance the public and private interests of America, and elevate our scholastic reputation in foreign countries.

Adam Seybert (1773-1825), of Philadelphia, was one of the very first Americans to enjoy a training in the School of Mines in Paris, which he attended during the closing years of the eighteenth century. In 1797, in the "Transactions of the American Philosophical Society," he printed "Experiments and Observations on Land and Sea Air." It is the first research of this kind. It relates to the results of twenty-seven analyses of air made at sea on a voyage across the Atlantic. These analyses were afterward compared with analyses of air near Philadelphia, and the conclusion drawn that the air over the sea is purer than the air over the land. Much credit is due Seybert



Samuel Latham Mitchill

for his study because the instruments he used must have been exceedingly crude and primitive. Seybert was one of the group of men who conducted the laboratory of the Chemical Society.

It was his son Henry who communicated to the American Philosophical Society an important paper giving analyses of the chrysoberyls both of Haddam in Connecticut and Brazil. The analyses reveal the interesting fact, not heretofore suspected, that the chrysoberyl contains *Glucina* to the amount of about 15 to 16 per cent. The same mineral was analysed in 1822, without detecting the Glucina, though it was sought for by that acute and promising chemist Mr. Arfvedson of Sweden. In the analyses by both chemists the mineral was repeatedly treated with caustic potassa; the insoluble residue, after each fusion, being again subjected to the action of the same alkali. In each analysis an insoluble residue, not attacked by the potassa, amounting to about one-sixth of the mineral employed, was obtained. The insoluble residue was found by Mr. Seybert to be Glucina associated with about a sixteenth of oxide of titanium; while, according to Arfvedson, "on examination, it proved to be pure silica."

CHAPTER VIII

A MEMORABLE discovery was made at this time—the invention of the compound blow-pipe, and Robert Hare, of Philadelphia, was the inventor. It is now called the oxy-hydrogen blowpipe. By its use wonderful results were obtained. It is a real landmark in scientific discovery.

It seems, therefore, most appropriate to insert the original language, descriptive of the invention, at this point:

MEMOIR
of the
SUPPLY AND APPLICATION
of the
BLOW-PIPE.

Containing

An Account of the new method of supplying the Blow-Pipe either with common air or oxygen gas: and also of the effects of the intense heat produced by the combustion of the hydrogen and oxygen gases.

ILLUSTRATED BY ENGRAVINGS.

Published by order
of the
CHEMICAL SOCIETY
OF PHILADELPHIA,
to whom
it was presented
BY ROBERT HARE, JUN.
Corresponding member of the Society.

PHILADELPHIA:
Printed for the Chemical Society,
By H. Maxwell, Columbia-House,
1802.

INTRODUCTION

On the 24th of October, 1801, a committee, of which I was a member, was appointed by the Chemical Society, for the discovery of means, by which a greater concentration of heat might be obtained for chemical purposes.

The committee thus appointed, soon after informed the Society, that as they had conceived, that the only way of attaining the object of their appointment, would be to precipitate more copious supplies of oxygen gas, into any focus of combustion; they had therefore confined their attention to the exhibition of a machine, by which this would be much facilitated.

This machine had been previously invented by me; and I was induced by the recommendation of my colleagues, to subject it to the attention of the Society.

The Society, honoured the report of the committee with a favourable reception; ordered them to procure an engraving of the machine; and that this, together with an explanation of it, should be laid before the public.

These commands of the Society, would have been long since complied with, but that experiments suggested themselves, the execution of which has demanded time. Some of these experiments appear to invalidate the opinion, that the precipitating of larger supplies of oxygen gas, into any focus of combustion, would be the only way, by which the intenseness of caloric could be increased to a degree not before attained.

On the tenth of last December, I informed the Society of my having conceived, that a more intense heat might be obtained by the united combustion of hydrogen, oxygen, and carbon, than had been before produced: and I, at the same time, laid before them an improvement of my machine, which tended much to facilitate the application of

the hydrogen, and oxygen gases to this joint combustion.*

Still continuing my experiments on that subject, I was afterwards enabled to produce to the Society, fused specimens of native lime, and pure magnesia; and to inform them, that barytes, alumine, and platina, were susceptible of rapid fusion.

An account of the result of these, and other experiments, on the supply and application of the Blow-Pipe; together with an engraving, and an explanation of the apparatus, by which they were effected; are the subjects of the following paper.

* Extract from the minutes of the Chemical Society of the tenth of December, 1801.

"Mr. Hare then called the attention of the Society to some improvements in his newly invented hydrostatic blow-pipe, by which he was enabled to exhibit the combustion of the hydrogen and oxygen gases, the heat thereby produced being very intense."

MEMOIR
of the
SUPPLY AND APPLICATION
of the
BLOW-PIPE.

CHAPTER I

IMPORTANT USES OF THE BLOW-PIPE—IMPERFECTIONS OF THE MEANS HITHERTO EMPLOYED, FOR SUPPLYING IT WITH AIR—INVENTION OF A MACHINE, FREE FROM THOSE IMPERFECTIONS.

The Blow-Pipe is, on many occasions, an useful instrument, to the artist and philosopher. By the former it is used, for the purpose of enamelling, to soften or solder small pieces of metal, and for the fabrication of glass instruments: while the latter can, by means of it, in a few minutes subject small portions of any substance to intense heat; and is thereby enabled to judge, of the advantages to be gained, and the method to be pursued, in operations on a larger scale. The celebrated Bergman has amply displayed the utility of this instrument, in docimastic operations; and with the perfection of the docimastic art, the improvement of metallurgy is intimately connected. It is by means of the Blow-Pipe, that glass tubes are most conveniently exposed to the heat necessary to mould them into the many forms occasionally required for philosophical purposes; and by the various application of tubes thus moulded. ingenuity is often enabled to surmount the want

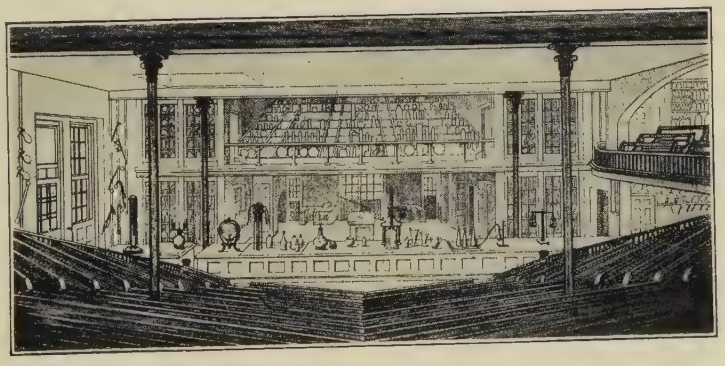
of apparatus, which is the greatest obstacle to the attainment of skill in experimental philosophy.

To all the purposes which I have mentioned, the Blow-Pipe is fully adequate, when properly supplied with air, and applied to a proper flame: but it appears that the means which have hitherto been employed to accomplish those ends, are all faulty.

The most general method, is that of supplying this instrument with the breath. In addition to the well known difficulty of keeping up a constant emission of air during respiration, and its injurious effect on the lungs;* it may be remarked, that as the breath is deprived of part of its pure air, is mixed with carbonic acid gas, and loaded with moisture, it is not the most fit for combustion; and the obvious impossibility of supporting a flame with oxygen gas, by this method, is also worthy of consideration.

Another way of supplying the Blow-Pipe with air, is that of affixing to it a small pair of double bellows. A contrivance of this kind possesses obvious advantages over the mouth Blow-Pipe; but, owing to the pervious nature of the materials of which bellows are constructed, and the difficulties of making their valves air tight, upwards of nine-tenths of the air drawn into them, escapes at other places than the proper aperture. A pair of bellows of this kind, belonging to an artist of this city, which were considered as unusually air tight, were found to discharge the complement of their upper compartment, in six-fourths of a minute, when the orifice of the pipe was open; and in seven-fourths of a minute, when it was closed. Hence it appears, that six-sevenths of the air injected into the upper compartment, escaped at other places than the proper aperture; and if to this loss, were added that sustained by the lower compartment, the waste would be found much greater. As in operating with these machines, it is nec-

* In consequence of this some artists have abandoned the use of the instrument.



HARE'S CHEMICAL LABORATORY

essary constantly to move the foot, the operator cannot leave his seat; and in nice operations, the motion of his body is an inconvenience, if not a source of failure. Bellows of this kind cannot be used for supplying combustion with oxygen gas; because, as this air is only to be obtained by a chemical process, the smallest waste of it is of serious consequence; and as there is always a portion of air remaining in them, even when the boards are pressed as near to each other as the folding of the leather will permit, any small quantity of oxygen gas which might be drawn into them, would be thereby contaminated.

It seems, that the only instrument hitherto used, for the supply of combustion with oxygen gas, is the gazometer of the celebrated Lavoisier: but this machine, although admirably calculated for the purposes of that great philosopher, is too inwieldy and expensive, for ordinary uses.

Being sensible of the advantages which would result, from the invention of a more perfect method of supplying the Blow-Pipe, with pure, or atmospheric air, I was induced to search for means of accomplishing this object. Having observed the cheapness, strength, and tightness of coopers' vessels, I became desirous of forming an apparatus for my purpose, by means of hydrostatic pressure, exerted within them. I soon found, that this could not be effected conveniently, without the use of leather. Obliged to resort for assistance to this material, I endeavored to apply it in such a manner, as to remedy the evils resulting from the use of it, in the common kind of bellows. The causes of these evils appeared to be, the opening of the pores and joints of these instruments, by dryness, and the tension to which they are so frequently subjected. I therefore determined to subject the leather, which I should use, to moisture and compression. In this I succeeded, and derived the expected advantage from success. The result of my attention to this subject, is the production

of a machine, of which there follows an engraving and description.

When it was first shown to the gentlemen of the Chemical Society, some of them bestowed on it the appellation of Gazometer; but, as etymology does not authorize this name, it has been changed for that of Hydrostatic Blow-Pipe.

CHAPTER II

EXPLANATION OF AN ENGRAVING OF THE HYDROSTATIC BLOW-PIPE—ACCOUNT OF THE MANNER, AND PRINCIPLE OF ITS ACTION.

Fig. 1 (see plate) is a perspective engraving of the Hydrostatic Blow-Pipe.—Part of this figure is made transparent, that the internal construction of the machine, may be understood with the greater facility.

It consists of a cask A, whose length is thirty-two, and whose least diameter is eighteen inches. It is divided, by the partition B, into two apartments. The upper, and external apartment B A, is in depth fourteen inches. The lower, and internal apartment, B C, is in depth sixteen inches; and contains a sheet and pipe of copper E E, D, which descends into it nine inches, forming two equal compartments of that depth. The sheet and pipe of copper are soldered together, and inserted into the partition B, as may be observed at Fig. 2; where B represents the partition; E E the sheet of copper; and D the pipe. The edges, E E, of the sheet, were slid down into corresponding joints in the staves of the cask, until the partition attained its proper situation. Coopers' flags were then passed into the joints; and the hoops were driven on the cask.

C F, Fig. 1, is a pair of circular bellows. The bottom of the cask, serves as a bottom for these bellows. In the cen-

ter of this bottom, there is a hole; round which, at the distance of one inch from its center, is a circular rim of wood. On this is nailed a valve opening upwards, which may be observed at B, Fig. 3, where there is a transparent engraving of the bellows. Under the valve B, may be observed the hole, and circular rim of wood, over which it is nailed.—C the top of the bellows, is a circular piece of wood, seven inches in diameter, and two in thickness. In its center there is a hole, one and a half inches in diameter. Around this hole there is a circular rabbet, in which is nailed a valve, opening upwards. This valve, and the rabbet in which it is fastened, may be seen under the letter D, at the end of the rod. There is also in this top, at the distance of one inch from its perimeter, a circular dovetailed furrow filled with lead E.

The body of the bellows F F, is composed of strong hose leather, sewed so as to be water-tight. Before it was fixed to the other parts of the bellows, its form was that of a hollow frustum of a cone; of which the perpendicular, and greatest diameter, were each eight inches; and whose least diameter, was six and a half inches. It was more easily fastened to its appendages, when of this conical form, than if it had been cylindrical. At the protuberances F F, it is distended by two iron rings, to which it is sewed fast.

F G, Fig. I, is an iron rod, by means of which, the top of the bellows may be raised or depressed. It passes up through the pipe D, to the handle G, which is worked by the hand, or with the foot, by means of the pendent stirrup. An enlarged view of this rod, and of the contrivance by which it is annexed to the top, may be seen at Fig. 3; where G D represents the rod, and H, H, H, H, flat pieces of iron branching from it. These are fixed to the circular rim K K, in such manner as to include the rim I I, of the same metal, which is screwed fast to the top of the bellows. Sufficient room is left, to allow the pieces H, H, H, H, and the rim

K, K, to move round without rubbing against the included rim I I, or the top of the bellows.

A copper hood, with an opening in one side, may be observed at L, Fig. 3. The rod G D is passed through the center of this hood, until the flat pieces of iron H, H, H, H, come in contact with the flat part of it. The hole in the center is then luted. The hood may be seen in its proper situation, at F, Fig. 1.

H I, Fig. 1, is a suction pipe half an inch in diameter. It passes under the cask, in the direction of the dotted lines at C, and turns up into the hole in the bottom of the bellows. This hole, which is of such size as to fit the tapering end of the pipe, is seen at Fig. 3, and has already been mentioned, together with a circular rim of wood, which being nailed round it, prevents the end of the pipe from touching the valve. The suction pipe has a conical mouth at I; into which is inserted occasionally, the pipe J, fastened to the hose and syphon K, L. The hose is made of leather, distended by hollow cylinders of tin, half an inch in diameter, and one inch in length. These were coated with tar, after which the leather was sewed over them.*

Fig. 1, M N O, m n o, are pipes of delivery, furnished with cocks at N, n, and conical mouths at O, o. Each of these pipes, communicates with one of the compartments on each side of the sheet and pipe E E, D.

In the partition B, may be observed the pipe Y furnished with a cock. Each end of this pipe, communicates with one of the compartments above mentioned.

P is a table affixed to the cask by means of irons, which are at pleasure slid into, or out of staples. One of these irons, and its staples, may be seen near the letter Q. They

* This hose may be made very perfect by tarring and covering it with leather a second time, the seams of the first and second coverings being placed on opposite sides. Flexible pipes thus prepared will be found useful for many other purposes besides that here mentioned.

are fastened to pieces of wood, which run lengthwise under the table, and which are so grooved, as to support a block of wood, which slides between them. Through this block passes the screw S; which slides backward and forward in the opening T R V.—The stand T V, which may be observed under the lamp, is loosely put on this screw; but is prevented from turning round with it, by the upright strip of wood T.

Having described the construction of the Hydrostatic Blow-Pipe, I proceed to an explanation of the principle, and manner of its action, and to a detail of the uses to which it may be applied.

Suppose that as much water were poured into the cask A, Fig. 1, as would fill the lower apartment, and rise above the partition B, one or two inches. Let Fig. 4, be a representation of the cask, when supplied with this necessary quantity of water. When the machine is at rest, the top of the bellows being loaded with lead, is depressed as low as the folding of the leather will permit, and the small space which remains in consequence of this folding, between the top of the bellows and the bottom of the cask, becomes filled with water, which leaks through the upper valve. Let the bellows be extended by depressing the handle at a. The upper valve will shut tight; and a quantity of water equal to the bulk, which the bellows will gain by extension, will rise through the pipe D, to the external apartment; and the weight of the atmosphere being removed from the top of the valve, in the bottom of the cask, the air will press through the suction pipe I H, lift this valve, and occupy the vacant space within the bellows. If the hand be then removed from the handle, the lead in the top of the bellows will again depress it, and the air drawn into them, being thereby compressed, will force open the upper valve, and ascend. During its ascent, it will receive a strong lateral tendency from the hood, which will make it pass out at the open side of the hood, into that

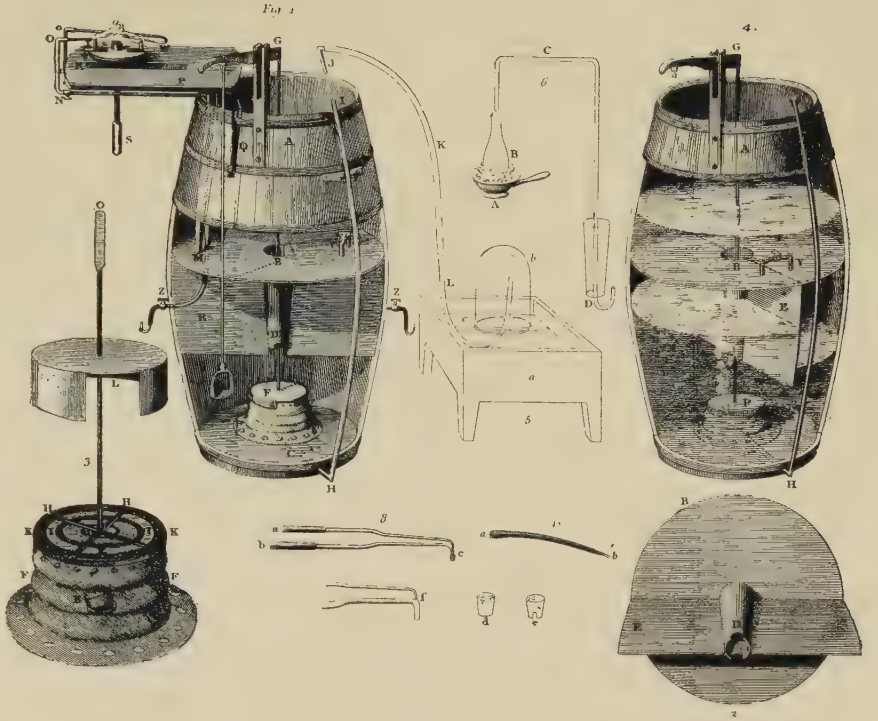
compartment which is immediately over this opening; and as by turning the rod, this part of the hood may be brought under either compartment, so the air may be thrown into either of them; and one of them being filled with one species of gas, the other may be filled with another species; nor can there be any danger of the mixture; because as the pipe D, is shorter than the sheet E E, any superabundant quantity of air, which may be thrown into either compartment, will pass up the pipe and escape.

In Fig. 4, the bellows are represented as nearly depressed; and the air issuing from the open side of the hood into the compartment immediately over it, which is about half filled with air. The other compartment is represented as being completely full of that fluid. The water is represented in commotion, that the action of the machine may be strongly marked; but the motion of this fluid is in reality so gentle, that the regularity of a blast is not thereby perceptibly affected.

If it be desired to fill both compartments with one kind of air, without the trouble of turning the hood; by opening the cock of communication in the pipe Y, any air which may be thrown into either compartment, will divide itself equally between both of them.

It must be obvious, that the air in the compartments on each side of the sheet and pipe of copper E E, D, Fig. 4, is subject to hydrostatic pressure; and that of course, it will pass out at the pipes of delivery unless stopt by the cocks. These pipes are omitted in Fig. 4, but have been already described, together with their cocks, at M N O, m n o, Fig. 1.

The leather and joints of the bellows, are evidently subjected to the weight of a considerable column of water; but this pressure being external, tends to tighten them, and renders this part of the machine so perfect, that if the orifice of the suction pipe be closed, it will be found impossible to raise the top of the bellows, without the im-



HARE'S HYDROSTATIC BALANCE

mense force which would be necessary to produce a vacuum within them. This would not be the case if the smallest leakage took place.

CHAPTER III

APPLICATION OF THE HYDROSTATIC BLOW-PIPE, TO THE USES OF THE MOUTH BLOW-PIPE, AND TO THE PURPOSES OF THE ENAMELLERS' LAMP—MANNER OF SUPPLYING IT WITH THE GASES—AND OF APPLYING IT TO THE SUPPLY OF COMBUSTION WITH OXYGEN GAS.

It is now time to give an account of the purposes to which the Hydrostatic Blow-Pipe may be applied, and the manner of applying it to them.

This instrument may be employed to supply with atmospheric air, a small flame for the various purposes of the mouth Blow-Pipe. To effect this, it is only necessary to place a lamp or candle, on the stand T V which is upheld by the screw S, Fig. 1. By raising or lowering this screw, or by sliding backward or forward, the block through which it passes, the stand may be so adjusted, so that the straight mouth piece X will just enter the flame. The handle must then be worked, until the blast obtains the proper strength. This generally happens when the water has risen above the partition B, three or four inches. If it should be raised higher, the blast may be regulated by turning the cock more or less at N.

When an operation is to be performed on a subject which cannot be held over the table: by fixing the small hose and Blow-Pipe a b, Fig. 7, into one of the conical mouths O, o, of the pipes of delivery, and by placing a lamp or candle on the edge of the table, an operator may with the subject in his hand expose the proper spot to the flame. In this

way glass matrasses filled with liquors, have been hermetically sealed.

Nothing can be more steady, than the stream of air emitted by this instrument. The falling off in pressure, arising from the descent of the water, does not perceptibly affect the flame, in a blast of six minutes duration; and in the meantime, the handle may be depressed so gently, that the most strict observation will not discover the least unsteadiness to be produced by it. Or if the machine be filled with air, by opening the cock more or less, an equable blast may be supported for more than the space of an hour.

In order to supply the enamellers' lamp with air by means of the Hydrostatic Blow-Pipe, it is only necessary to substitute this instrument, for the bellows commonly used for this purpose. There will then be nothing novel in the manner of operating, excepting, 1st. That the relative situation of the flame and the pipe is to be regulated by turning the screw S, or by sliding backward or forward, the block through which it passes; and, 2dly. That in lieu of the frequent movement of the foot, necessary with the common bellows; in the space of one minute, and with fifteen strokes of the handle, as much air may be drawn into the Hydrostatic Blow-Pipe as will blow for one hour; and as the casks and pipes are completely airtight, the blast may be stopt, or its strength increased or diminished at pleasure, by turning, more or less the cock of the pipe delivering the air.

The flame of the enamellers' lamp is not used exclusively, for the purposes of the artist from whom it takes its name. It is this modification of the principle of the Blow-Pipe, which is applied to the moulding of glass instruments. But in heating glass with this flame, an inconvenience arises from the impossibility of exposing both sides of any subject to the same heat, unless it be constantly turned round; for if only one side of a large glass tube be applied to the

flame, the part exposed to its action will be fused, before the other will be softened, and if it be turned round constantly, a much longer time will be required to melt it. Indeed some large tubes of refractory glass, which are not to be melted while undergoing this rotary motion, may be readily fused in any spot constantly exposed to the action of the flame.

In order to produce a flame which should be free from the inconveniences just described, I procured the oblong lamp with two wicks W, X, Fig. 1. It may be observed, that these wicks are fixed on two plates, which slide in a groove, in the direction of the length of the lamp. They may therefore be made to approach to, or recede from each other. This lamp being as represented in the engraving placed on the little stand T V, so as that one of the wicks was before the orifice of the straight mouth piece, above X; the bent Blow-Pipe at W was so adjusted to the other wick, that when they were both lighted, and a blast passed over them, their flames met each other as represented in the plate. The result of this was, that a much larger tube could be fused by the united action of two flames, than could be melted with one of them; and the parts being more equally heated, a bend could be made more regularly, and with less danger of collapsing.

It may be proper to observe, that the machine represented in the plate is much more complex and expensive, than is requisite for the purposes of the mouth Blow-Pipe, or enamellers' lamp, simply. But it is expected that artists availing themselves of the principle of the machine, will reject those appurtenances which are not necessary to their peculiar purposes.*

The Hydrostatic Blow-Pipe may be filled with any of

* The cost of the machine represented in the plate was about twenty dollars, but a machine fully equal to the purposes of the mouth blow-pipe or enameller's lamp may be made for one-fifth of that sum.

the gases, by exhausting them from the inverted jars, of the pneumatic-chemical apparatus: and if it be desired to confine different species of gas, by closing the cock of communication between the compartments; one of them may be filled with one kind of gas, and afterwards by turning the hood, the other compartment may be filled with another kind. To make this understood; let a, Fig. 5, be a pneumatic-chemical tub, with a shelf c, and an inverted glass jar b. Suppose that the tub were filled with water, and jar with gas. Lute the pipe J, Fig. 1, to the mouth of the suction pipe at I; pass the syphon L under the jar as may be observed in Fig. 5, and then extend the bellows. The bellows will become filled with the air of the jar, and this being discharged into that compartment of the cask, which is over the open side of the hood, the bellows will be ready for another extension; the repetition of which would soon exhaust the jar of its air, although it should be of the largest size.

This method of filling the machine is very convenient in a laboratory well supplied with pneumatic-chemical apparatus. But it is a principal convenience of the Hydrostatic Blow-Pipe, that it may be filled with any gas, immediately from the retort, bottle, or matrass, made use of in obtaining it. Let D, Fig. 6 be a separate representation of the pipe D, Fig. 1. Let B be a matrass containing the substance from which the air is to be obtained, and let C be a syphon luted to the neck of the matrass. The air issuing from the matrass, must be emitted from the mouth of the syphon at the lower end of the pipe D. Suppose that this pipe were in its proper situation at D, Fig. 1. The air issuing from the matrass, would be discharged into that compartment of the cask, under which the mouth of the syphon should be placed, and if the cock at Y should be closed, this compartment alone would become filled; but if this cock should be open, the air would divide itself equally between both compartments. It must be obvious,

that while one matrass and syphon, are employed in filling one compartment, with one species of air, the bellows, or another matrass and syphon filled with different substances, may be employed in filling the other compartment with another species of air; and thus the oxygen, and hydrogen gases or oxygen gas, and atmospheric air, may at the same time be confined in the same vessel, without their mixing with each other.

Those who desire to experiment largely with oxygen gas, will find it advantageous, to make use of a cast iron matrass, with a short and large neck narrowing inwards, and about fifteen inches of a gun barrel. The neck of the matrass being made large or short, it will not only be easily filled, but it will be readily freed from any caput mortuum which may be left in it. The gun barrel must be ground to fit the neck of the matrass.

The syphon for conveying the gas into the cask, may be fitted to the gun barrel with a cork.

The philosophical world has been for some time acquainted with the intense heat produced by combustion supported with oxygen gas. By means of the Hydrostatic Blow-Pipe, every artist may, with little trouble and expense, avail himself of the intense heat produced by this combustion.*

Probably there are not at present many operations in the arts, which require greater heat than may be produced by the ordinary means; but it is certain, that the knowledge of a process cannot precede an acquaintance with the heat necessary to effect it; and this most intense fire, being placed within the reach of the artist, it is highly probable,

* In a former page I mentioned the gazometer of Lavoisier as being too complicated for ordinary application to the supply of oxygen gas—I should also have noticed the apparatus of Sadler and the gazometer of Seguin, but, if I am not mistaken, these, although very ingenious inventions, are liable to the same objection.

that cases may be discovered, in which it may be applied with convenience and utility.

The most convenient way of making use of oxygen gas for small operations, is to supply one of the compartments of the Hydrostatic Blow-Pipe with that gas; to retain the gas thus confined for those moments when the greatest heat is required; and by means of the other compartment, to make use of atmospheric air when the heat produced by it is sufficiently intense. It must be obvious, that if the conical mouths, O, o, of the pipes M, N, O, m, n, o, Fig. 1, be furnished with straight mouth pieces, that any lamp or candle placed on the stand T V, may be readily shifted from one mouth piece to the other, when it shall be desired to expose any subject successively, to the heat produced by atmospheric air, and oxygen gas.

If it be wished to make use of the heat produced in the combustion of charcoal with oxygen gas, after having confined a sufficient quantity of this gas; it will be necessary to fix in the conical mouth of the pipe, communicating with the compartment containing the gas, the larger end of a common brass Blow-Pipe, the orifice being directed downwards. Under this orifice, the body to be acted upon must be placed, supported by a piece of charcoal, in the form of a parallelopiped, the charcoal being ignited in the part contiguous to the body. Things being thus arranged, by turning more or less, the cock of the pipe in which the Blow-Pipe shall be fixed; a stream of oxygen may be precipitated on the burning spot, with the proper degree of rapidity.*

This method of supporting the combustion of carbon

* In detailing the uses of the hydrostatic blow-pipe, it may be proper to mention the facility which it gives to the employment of the gases for medical purposes. When this machine is filled with any gas, the bag to be made use of in respiring it may be inflated by fixing it to the mouth of the pipe of delivery, communicating with the gas.

with oxygen gas, is nearly the same as that by which the celebrated Lavoisier performed his experiments; excepting that in the place of the Hydrostatic Blow-Pipe, he made use of his Gazometer.

CHAPTER IV

EVILS EXPERIENCED IN OPERATING WITH THE COMBUSTION OF CARBON AND OXYGEN GAS—SUPERIOR HEAT OF COMBUSTION SUPPORTED BY THE HYDROGEN AND OXYGEN GASES—ITS EFFECT ON THE MOST REFRACTORY SUBSTANCES.

In the introduction to this paper, it was mentioned, that some experiments had been performed, which seemed to invalidate the opinion that the employment of larger quantities of oxygen gas, would be the only means of increasing the power of caloric. I shall proceed to give an account of these experiments; but will first retrace the ideas which led to them.

In operating with the combustion of carbon and oxygen gas, great evils were observed to result, from the difficulty of placing the subject of the operation in the focus of the heat, without interrupting the stream of air by which this heat was supported. Not only was the focus widened by this interruption, and the intenseness of the heat thereby lessened; but the stream of air oxydated those substances which were combustible, and cooled those which were otherwise, in the places where it impinged previously to its union with the charcoal. Added to this, the charcoal was so rapidly consumed, that the substance acted on became so much buried, that it was difficult to follow it with the eye, or the orifice of the pipe: and some substances were observed to run into the pores of the coal, and elude examination.

To avoid these evils it was thought desirable, that means might be discovered, of clothing the upper surface of any body which might be subjected to this species of operation, with some burning matter, of which the heat might be equal to that of the incandescent carbon, with which the lower surface might be in contact: or by which bodies might be exposed on solid supports to a temperature, equal or superior to that of the porous charcoal uniting with oxygen.

It soon occurred, that these desiderata might be attained by means of flame supported by the hydrogen and oxygen gases; for it was conceived that according to the admirable theory of the French chemists, more caloric ought to be extracted by this, than by any other combustion.

By the union of the bases of the hydrogen and oxygen gases, not only is all the caloric of the oxygen gas evolved; but also a much larger quantity which must be necessary to give the particles of the hydrogen their superior power of repulsion. The product of this combustion is water in the state of steam which retains heat so slightly, that it acts merely as a vehicle to deliver it to other bodies. What is necessary to preserve to water its form of fluidity, is the only portion of the caloric extricated in this combustion, which is permanently abstracted.

The combustion of carbon with oxygen gas, has been hitherto considered as the hottest of all fires. The caloric evolved in this case proceeds from the oxygen gas alone, while the product of this combustion is carbonic acid gas, which abstracts the large quantity of caloric, necessary to give it the form of permanent air, but which adds nothing to the heat of the combustion. Hence it is evident, that more caloric is evolved, and less abstracted, in combustion supported by the hydrogen and oxygen gases, than in that supported by oxygen and carbon.

However the intenseness of the heat of combustion, is not only dependent on the quantity of caloric extricated;

but also on the comparative smallness of the time, and space, in which the extrication is accomplished. But in this respect the aeriform combustible, has obviously the advantage over those which are solid, as its fluid and elastic properties, render it susceptible of being rapidly precipitated into the focus of combustion, and of the most speedy mixture with the oxydating principle when arrived there.

The opinion of the intenseness of the heat produced by the hydrogen and oxygen gases, thus upheld by theory, derives additional support from the practical observation, of the great heat of a flame supported by hydrogen gas while issuing from a pipe; and also of the violent explosion which takes place, when it is mixed with oxygen gas and ignited, for it appears that this explosion can only be attributed to the combination of an immense quantity of caloric, with the water which is either held in solution by these gases, or formed by the union of their bases.

Such was the reasoning, which originated the desire of employing the flame of the hydrogen and oxygen gases. Before this could be accomplished, it was necessary to overcome the difficulty of igniting a mixture of these aeriform substances, without the danger of an explosion. It was for the purpose of surmounting this difficulty, that the Hydrostatic Blow-Pipe was furnished with two compartments; by means of which the machine might be at the same time charged with different species of air, without any possibility of mixture. One of these compartments being supplied with oxygen, and the other with hydrogen gas; two common brass Blow-Pipes a, b, Fig. 8, were joined at their orifices to two tubular holes in the conical frustum of pure silver c, of which the mean diameter is one-third, and the length is three-fourths of an inch. The diameter of one of these holes is large enough for the admission of a common brass pin. The other hole is a third less. They commence separately on the upper surface of the silver

frustum near the circumference, and converge so as to meet in a point, at the distance of a line and a half from the lower surface. In the space between the lower surface, and the point of meeting, there is a perforation of the same diameter as the larger hole. The manner in which this perforation and the tubular holes communicate one with the other, may be understood from the lines in the form of the letter Y, in the transparent representation of the silver conical frustum at d. The pipes a b were then fitted into the mouths O, o, of the pipes of delivery, Fig. 1: so that the Blow-Pipe inserted into the larger hole of the frustum, should communicate with the compartment containing the hydrogen gas; and that the other should communicate with that, which contained the oxygen gas. The cock of the pipe communicating with the hydrogen gas, was then turned until as much was emitted from the orifice of the cylinder, as when lighted formed a flame smaller in size than that of a candle. Under this flame was placed the body to be acted on, supported either by charcoal, or by some more solid, and incombustible substance. The cock retaining the oxygen gas, was then turned until the light and heat appeared to have attained the greatest intensity. When this took place, the eyes could scarcely sustain the one, nor could the most refractory substances resist the other.

However, it is worthy of notice, that the light and heat of this combustion do not become evident until some body is exposed to it, from which the light may be refracted, or on which the effect of the heat may be visible. This is not the case with combustion supported by oxygen and carbon; for no sooner is a stream of oxygen gas directed on ignited carbon, than an effulgence is produced, which impresses the mind of the beholder with an idea of the greatest heat being produced by it.

It is in this different appearance, of these different species of combustion, that we may discover the reason why

philosophers have neglected the one; while they have bestowed much attention on the other.*

In lieu of the conical frustum represented at c d; that at d e may be used. The tubular holes of this last mentioned frustum, do not meet, but deliver their air at separate orifices into an excavation in the lower part of the frustum. The dotted lines represent the tubular holes; and the arched line the excavation. This is about three lines in diameter, and enters into the silver about the same distance.

At f, are represented pipes which are used for the fusion of platina, or subjects of the larger kind. They consist of a large, and a small pipe, the orifice of the one, being inserted into that of the other; as may be understood from the dotted lines near f.

The purity of the gases contained in the Hydrostatic Blow-Pipe, may be at any time examined by charging eudiometers, from the syphon and leathern pipes hanging to the cocks Z Z Fig. 1. These cocks are soldered to curved pipes, one of which is represented in the figure. By turning the cocks round, the mouths of the curved pipes may be brought down to the surface of the water; this gives a facility to the discovery of any heavier gas, which may be mixed with one which is more light; as the fluid whose specific gravity is greater, will be found on the surface of the water.

I shall now describe the changes effected on the most

* The inferiority of the light emitted by the flame of the hydrogen and oxygen gases to that which irradiates from bodies exposed to its action adds one to the many instances in combustion, in which the quantity and color of the light extricated do not seem to be so much dependent on the quantity of oxygen gas consumed as on the nature of the substances heated or burned. In this, therefore, we may find support for the idea that the light extricated by fire, or emitted by heated bodies, proceeds not only from the decomposition of pure air, but from that of the combustible or the heated bodies themselves.

fixed and refractory substances, by the flame of the hydrogen and oxygen gases.

In order to avoid a tedious recurrence to an awkward phrase, I shall generally in the subsequent part of this paper distinguish the flame of the hydrogen and oxygen gases, by the appellation of gaseous flame.

By exposure to the gaseous flame, either on supports of silver, or of carbon; barytes, alumine, and silex, were completely fused.

The products of the fusion of alumine and silex, were substances very similar to each other, and much resembling white enamel.

The result of the fusion of barytes, was a substance of an ash coloured cast, which after long exposure sometimes exhibited brilliant yellow specks. If it be certain that barytes is an earth, these specks must have been discoloured particles of the silver support, or of the pipes from which the flame issued.

Lime and magnesia are extremely difficult to fuse, not only because they are the most refractory substances in nature, but from the difficulty of preventing them from being blown on one side by the flames; nevertheless, in some instances by exposure on carbon to the gaseous flame, small portions of these earths were converted into black vitreous masses. Possibly the black colour of these products of fusion, may have been caused by iron contained in the coal; for in the high temperature of the gaseous flame, a powerful attraction is reciprocally exerted by iron and the earths.

Platina was fused by exposure on carbon, to the combustion of hydrogen gas and atmospheric air. But the fusion of this metal was rapidly accomplished by the gaseous flame, either when exposed to it on carbon, or upon metallick supports.

A small quantity of this metal in its native granular form, being strewed in a silver spoon, and passed under

the gaseous flame; the track of the flame became marked by the conglutination of the metal: and when the heat was for some time continued on a small space, a lump of fused platina became immediately formed.

About two penny-weights of the native grains of platina, when subjected to the gaseous flame on carbon, became quickly fused into an oblate spheroid as fluid as mercury. This spheroid after being cooled was exposed as before. It became fluid in less than the fourth of a minute.

Had I sufficient confidence in my own judgment, I should declare, that gold, silver, and platina, were thrown into a state of ebullition by exposure on carbon to the gaseous flame; for the pieces of charcoal on which they were exposed became washed or gilt with detached particles of metal, in the parts adjoining the spots, where the exposure took place. Some of the particles of the metal thus detached, exhibited symptoms of oxydation.

As the fusion of lime and magnesia by exposure on carbon, was accomplished with great difficulty and uncertainty: it became desirable, that means might be discovered of effecting this fusion with greater ease.

By the union of the base of oxygen with iron, the whole of the caloric of this elastic fluid is supposed to be extricated. This consideration, together with some practical remarks on the heat of burning iron, induced me to employ the combustion of this metal, in conjunction with that of the hydrogen and oxygen gases.

Some pieces of iron wire, each of about half an inch in length, were quickly thrown into fusion and rapid combustion, by exposure on carbon to the gaseous flame. When either lime, magnesia, barytes, alumine, or silex, were thrown on the iron in this state, they became instantly melted and incorporated with the metal. It remains a question whether in this case the earths were fused or dissolved; and whether the substances which resulted from their union with the iron, were mixtures, or combinations.

If they were combinations, according to the present nomenclature, they should be denominated *ferrurets.*

The difficulty of uniting some substances which are only susceptible of combustion at very high degrees of heat, has hitherto excluded them from the laboratory. By means of the gaseous flame, such substances may be employed with the greatest facility, in small analytical operations.

Of course the nature of the substances above described are the carburets of iron; and some peculiar species of native coal.

Among the carburets of iron, the English plumbago is esteemed the best. Some pieces of this substance obtained from the best English black-lead pencils, were readily thrown into combustion by exposure to the gaseous flame, either on carbon, or on some larger pieces of American plumbago. It was found that either lime or magnesia were fusible when exposed to the fire thus produced. This however, may have been caused by the iron contained in the carburet, for the fused earths, and plumbago, generally adhered to each other.

There is a peculiar species of native coal found on the banks of the Lehigh in this state, which it is extremely difficult to ignite: but when exposed to a high degree of heat and a copious blast of air, it burns yielding an intense heat without either smoke or flame, and leaving little residue. By exposure to the gaseous flame on this coal both magnesia and lime exhibited strong symptoms of fusion. The former assumed a glazed and somewhat globular appearance. The latter became converted into a brownish semivitreous mass.

The heat of the gaseous flame is very much dependent on the proportional quantities of the gases emitted. On this account the perforations in the keys of the cocks N, n, Fig. 1st, should be narrow and oblong to admit of a more gradual increase or diminution in the quantity of gas emitted.

I have now concluded my communications on the subject of this paper, and shall be happy if they have been found worthy of the time and attention bestowed on them by the society.

Benjamin Silliman, the elder, developed a great many of the excellent points of Hare's blowpipe and made a number of improvements in the apparatus, and, therefore, it seems particularly appropriate to note how Silliman's attachment to Hare began. A few paragraphs will be taken from his biography:

I arrived in Philadelphia at the close of a season of yellow fever, having never been there before. The city was comparatively deserted; the streets were quiet, and an air of anxiety was visible in the aspect of the remaining citizens. Still, as cool weather had commenced, no serious danger was apprehended, and by the recommendation of my friends, ... I engaged lodgings at Mrs. Smith's, corner of Dock and Walnut streets. Dock Street runs diagonally from the river, crossing Walnut Street at an acute angle, and there a wedge-shaped house had been erected which was now to be my home for four months, both in this year and the next.

This house attracted a select class of gentlemen. The Connecticut members of Congress resorted to it, I believe, while the government was in Philadelphia; and after its removal, as they were passing to and from Washington, it was a temporary resting place. Other gentlemen of intelligence were among its inmates, and several of them, being men of great promise, were then rising into the early stages of that eminence which they attained in subsequent years. Among them were Horace Binney, Charles Chauncey, Elihu Chauncey, Robert Hare, John Wallace and his brother; and as frequent visitors, John Sargeant and George Vaux. There were occasionally other gentlemen,

but those I have named were our stars. Alas! of the eight whom I have named only two remain; and if I add myself,—then an almost unknown young man,—the circle of names will be nine, and the survivors three,—Horace Binney, Robert Hare, and B. Silliman. Horace Binney, Charles Chauncey and John Sargeant rose to the head of the Philadelphia Bar, and John Sargeant was afterwards a member of Congress, and, I believe, of the Senate of the United States. Robert Hare took first rank as a chemist and philosopher; Elihu Chauncey was an eminent banker and financier, and the Wallaces and Vaux were most agreeable gentlemen,—Vaux, a Quaker, but warm-hearted and of easy, polished manners. Enos Bronson, of Connecticut and Yale College, was also of our number. He edited the "United States Gazette" with much talent.

The gentlemen whom I have named, with the friends and visitors that were by them attracted to the house, formed a brilliant circle of high conversational powers. They were educated men, of elevated position in society, and their manners were in harmony with their training. Rarely in my progress in life have I met with a circle of gentlemen who surpassed them in courteous manners, in brilliant intelligence, sparkling sallies of wit and pleasantry, and cordial greeting both among themselves and with friends and strangers who were occasionally introduced. Our hostess, Mrs. Smith, a high-spirited and efficient woman, was liberal almost to a fault, and furnished her table even luxuriously. Our habits, were, indeed, in other respects far from those of teetotalers. No person of that description was in our circle. On the contrary, agreeably to the custom which prevailed in the boarding houses of our cities half a century ago, every gentleman furnished himself with a decanter of wine,—usually a metallic or other label being attached to the neck, and bearing the name of the owner. Healths were drunk, especially if stranger guests were present, and a glass or two was not considered

excessive,—sometimes two or three, according to circumstances. Porter or other strong beer was used at table as a beverage. As Robert Hare was a brewer of porter and was one of our number, his porter was in high request, and indeed it was of an excellent quality. I do not remember any water-drinker at our table or in the house, for total abstinence was not there thought of except, perhaps, by some wise and far-seeing Franklin.

Accustomed to a simple diet in New Haven, without wine or porter, and perhaps with only cider at dinner, the new life to which I was now introduced did not agree well with my health. Occasionally, vertigo disturbed my head, and the nervous system was affected. At the end of both seasons in Philadelphia I had made some progress towards incipient gout. On my knuckles, what appeared to be chalky concretions began to form, which however went away after my return to New Haven and to my usual mode of living. In the upper classes of society in Philadelphia, the habits of living were then very luxurious and the spirit worldly. In my case the effects of luxurious living were to a degree counteracted by vigorous exercise. Often I walked with my friend Charles Chauncey, even in severe weather and before breakfast, to the river Schuylkill, two to two and a half miles, and of course four to five miles out and back; and Robert Hare's brewery, one and a half mile up town, often gave the occasion of useful exercise: he became a warm friend to me. There were no outward manifestations of religion in our boarding-house. Grace was, I believe, never said at table, nor did I ever hear a prayer in the house. I trust that private personal prayers ascended from some hearts and lips, in a house where so many were amiable and worthy, although without a religious garb. On the Sabbath, some of our gentlemen resorted to the churches, and some dined out on that day.

The deficiencies of Dr. Woodhouse's courses were, in a

considerable degree, made up in a manner which I could not have anticipated. Robert Hare, my fellow-boarder and companion at Mrs. Smith's, was a genial, kind-hearted man, one year younger than myself and was already a proficient in chemistry upon the scale of that period: and being informed of my object in coming to Philadelphia, he kindly entered into my views and extended to me his friendship and assistance. A small working laboratory was conceded to us by the indulgence of our hostess, Mrs. Smith, and we made use of a spare cellar kitchen (a wedge-shaped house at the intersection of Walnut by Dock St. at the Southwest corner) in which we worked together in our hours of leisure from other pursuits. Mr. Hare had, one year before, perfected his beautiful invention of the oxy-hydrogen blow-pipe, and had presented the instrument to the Chemical Society of Philadelphia. His mind was much occupied with the subject, and he enlisted me into his service. We worked much in making oxygen and hydrogen gases, burning them at a common orifice to produce the intense heat of the instrument. Hare was desirous of making it still more intense by deriving a pure oxygen from chlorate of potassa, then called oxy-muriate of potassa. Chemists were then ignorant of the fact that, by mixing a little oxide of manganese with the chlorate, the oxygen can be evolved by the heat of a lamp applied to a glass retort. Hare thought it necessary to use stone retorts with a furnace-heat; the retorts were purchased by me at a dollar each, and, as they were usually broken in the experiment, the research was rather costly; but my friend furnished experience, and, as I was daily acquiring it, I was rewarded, both for labor and expense, by the brilliant results of our experiments. Hare's apparatus was ingenious, but unsafe as regards the storage of the gases. Novice as I was, I ventured to suggest to my more experienced friend that by some accident or blunder the gases—near neighbors as they were in their contiguous apartments,—might become mingled, when, on

lighting them at the orifice, an explosion would follow. I was afterwards informed, although not by Hare, that this accident actually happened to him, although with no other mischief than a copious shower-bath from the expulsion of the water. Many years afterwards, Professor Hitchcock at Amherst, from the same cause, met with an explosion which gave him a great shock, and for a time greatly impaired his hearing.

Silliman, at a later time, wrote as follows concerning his friend:

Robert Hare was born in Philadelphia, Jan. 17, 1781. His father was an Englishman, a man of strong mind, and honored in his adopted country by the public confidence. His mother was from a distinguished Philadelphia family. In early life he managed the business of an extensive brewery which his father had established, but his strong leaning toward physical science very early manifested itself and soon led him to abandon the pursuits of a manufacturer and devote his talents and fortune to science. Before the age of twenty he gave evidence of this predilection for scientific pursuits by following the courses of lectures on chemistry and physical science in his native city, and by uniting himself with the Chemical Society of Philadelphia, then embracing the names of Priestley, Seybert and Woodhouse.

Concerning the discovery of the blowpipe, the following is an interesting addition:

In 1801, Robert Hare communicated to the Chemical Society of Philadelphia a description of the oxy-hydrogen blowpipe which he then called a "hydrostatic blow-pipe." Silliman, having been much engaged with him in a series of experiments with this instrument in 1802-3, subsequently distinguished it as the "compound blowpipe" having, in fact, on his return from Philadelphia in 1803, constructed.

for the laboratory of Yale College the first pneumatic trough combining Dr. Hare's invention; an apparatus subsequently figured and described by Dr. Hare in his memoir "on the fusion of strontia and volatilization of platinum."* His memoir to the Chemical Society (p. 153) was separately published in 1801 and was republished in "Tilloch's Phil. Mag., London, 1802," and also in the "Annales de Chimie (1st series)" V. 45.

This apparatus was the earliest and perhaps the most remarkable of his original contributions to science. It was certainly evidence of a highly philosophical mind, that Hare, in that comparatively early period in modern chemistry, and when the received notions of the true nature of combustion were so vague, not to say erroneous, should have had the acumen to conceive that a stream of oxygen and hydrogen burning together should produce so intense a heat. Lavoisier, certainly one of the most acute of chemical philosophers and unsurpassed in his skill as an experimentalist, had beaten up the same path so far as to direct a jet of oxygen upon charcoal, and he thus produced a degree of heat by which he fused alumina and other bodies before deemed infusible. He had even brought the elements of water into the same vessel and had there burned them from separate jets, in his famous apparatus for the recomposition of water. But it seems never to have occurred to him that here was a source of heat greater than any then known. In our view Hare's merit as a scientific philosopher is more clearly established upon this discovery than upon any other of the numerous contributions he has made to science. His original experiments were repeated in 1802-3 in the presence of Dr. Priestley, the discoverer of oxygen, then on a visit to Philadelphia, and of Silliman, Woodhouse, and others. They were subsequently greatly extended by Silliman, who, with the apparatus already alluded to, subjected a great number of refractory bodies

* Trans. Amer. Phil. Soc., *6*, p. 99, and plate 3 (June 17, 1803).

to the action of the oxy-hydrogen jet and published an account of his results in the Memoirs of the "Conn. Acad.," May 7, 1812.

The discovery of the oxy-hydrogen blowpipe was crowned by the American Academy at Boston with the Rumford medal.

The historian of science will, in view of the facts here quoted, find it needless to notice the disingenuousness of Dr. Clarke of Cambridge, England, in his "gas blowpipe," to overlook or appropriate the discovery of Hare and the researches of Silliman and others, several years after (in 1819) this discovery had been fully before the scientific world,—an effect which must ever remain as a sad stain upon the reputation of this otherwise distinguished man.*

It is not our purpose here to rehearse the history of Clarke's discovery in full, much less to describe all the modifications which the apparatus has received at the hands of its original discoverer and others. It is well known that in later years he constructed the apparatus on a gigantic scale, with large vessels of wrought iron capable of sustaining the pressure of the Fairmount water works, and that with this powerful combination he was able to fuse at one operation nearly two pounds of platinum.† In these experiments the metal is held upon a refractory fire brick and both are heated as highly as possible in a wind furnace

* The reader will peruse with interest, in this connection, Hare's elaborate defence of his own claims and those of his associate, Silliman, against Clarke's appropriation, in the American Journal of Science (1) V. 2, pp. 281-302, 1820. Clarke, after a full and spirited protest had been communicated to him, stating fully Hare's claims and the wrong done him, failed to make any acknowledgment of his error, thus exonerating us from the old maxim, "Nil de mortuis nisi bonum." Hare heads his strictures on Clarke's book with the well-known lines of Virgil, "Hos ego versiculos feci, tulit alter honores," etc.

† Roberts in New York has lately with Hare's apparatus succeeded in fusing perfectly 53 oz. of platinum at one operation.—N. Y. *Tribune*, 5/19.

before submitting it to the gas jet. The product of this fusion from the crude grains is found to be greatly purified, a result probably due to the volatilization at this intense heat of some of the associate metals.

The employment of Hare's jet to illuminate light houses and signal reflectors under the names of Drummond light and Calcium light is only another example of the mode of ignoring the name of the real discoverer, of which the history of science presents so many parallels.

The fertility of Hare's inventive mind is illustrated by the numerous and ingenious forms of apparatus which he contrived for research or illustration. To many of these he was led by the necessity of preparing the illustrations for his lectures upon a scale of magnitude adequate to the instruction of the large classes of the Medical School of the University of Pennsylvania. He was called to fill the Chair of Chemistry in that institution in 1818, and continued in the discharge of its duties for nearly a third of a century, and until his resignation in 1847.

He was fond of graphic illustrations; they abound in his memoirs and in his Compendium and other works, and aided by his lucid descriptions his inventions thus become quite intelligible. Where most instructors are satisfied with less perfect and more simple means and explanations, he seemed to be content with nothing short of perfection.

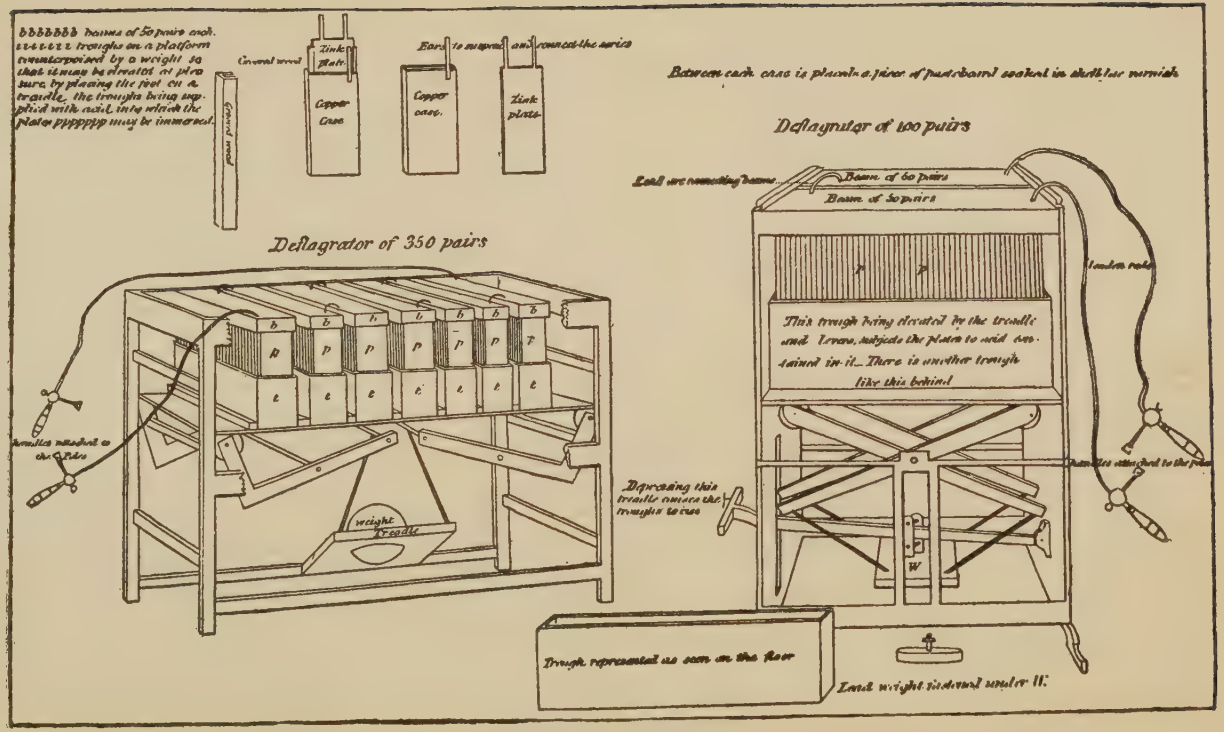
During his long course of research and experimenting, he accumulated a vast store of instruments and materials. An inspection of his repositories and the treasures there accumulated filled the observer with astonishment, and in his lecture room there was always a profusion of apparatus, often instruments of great dimensions, corresponding well with his large mind, with his great physical and intellectual power and unquenchable ardor. He was himself an able and skillful mechanic, and often worked adroitly at the turning lathe and with the other resources of a well furnished shop. In his operations he spared

neither labor nor expense, and bestowed both munificently for the accomplishment of his objects.

He devoted great labor and skill to the construction of new and improved forms of the voltaic pile, and it is easy to show that owing to his zeal and skill in this department of chemical physics American chemists were enabled to employ with distinguished success the intense powers of extended series of voltaic couples long in advance of the general use of similar combinations in Europe. In place of the cumbrous and unmanageable Cruickshank troughs with which Davy discovered the metallic bases of the alkalies, Hare introduced his *Deflagrator* (see picture), in which any series, however extended, could be instantaneously brought into action or rendered passive at pleasure. The peculiarities of Hare's deflagrators are too familiar to need any description here. Although the discovery of the constant battery by Daniell, and of the double combinations of platinum or carbon with amalgamated zinc and nitric acid have rendered the old forms of this instrument no longer so useful as formerly, it is not less a proof of the merit of Hare's apparatus that Faraday, in 1835, after having exhausted his ingenuity and experience in perfecting the voltaic battery, found that Hare had already, nearly twenty-five years before, accomplished all that he had attempted, and with a noble frankness worthy of all praise, he at once adopted Hare's instrument as embodying the best results then possible.*

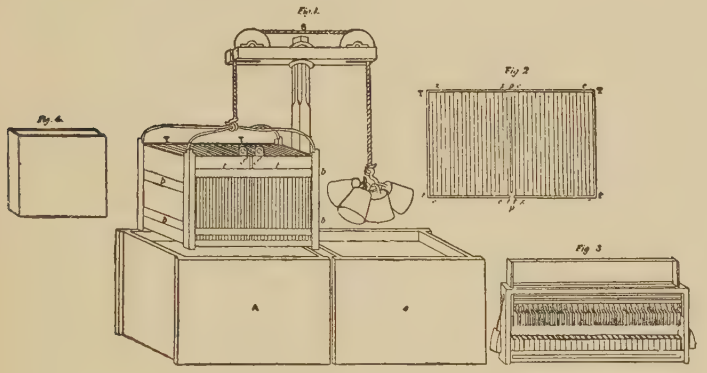
It was with one of Hare's deflagrators that Silliman, in 1823, first demonstrated the volatilization and fusion of carbon, a result considered so extraordinary at the time that it was long received with incredulity. Since the general introduction of Bunsen's battery these effects are no longer doubted; all of Silliman's results have been confirmed and extended by Despretz, De La Rive and others.

* Faraday's Experimental Researches, 1124, 1132.


Deflagrator of 350 pairs
Deflagrator of 100 pairs
Between each case is placed a piece of pasteboard soaked in shellac varnish
Zinc plate
Copper Case
Zinc plate
Copper case.
Beam of 50 pairs
Beam of 50 pairs
Lead wire connecting beams
Leaden rods
This trough being elevated by the treadle and screw, subjects the plates to acid contained in it. There is another trough like this behind
Depressing this treadle causes the troughs to rise
Trough represented as seen on the floor
Lead weight to be used under it.

HARE'S DEFLAGRATOR.

The deflagrator was invented in 1820.* Four years earlier Hare had contrived another instrument which he called the *Calorimotor*. In this instrument great extent of surface was obtained from the combining of many large plates (18″ or 24″ square) of zinc and copper into two series and plunging the whole at one movement into a tank



Hare's Calorimotor.

of dilute acid. The magnetic and heating effects of this instrument were surprising, and to this day no other form of voltaic apparatus appears to occasion the movement of so great a *volume* of heat with so low a projectile or intensive force. By it, large rods of iron or platinum, when clamped between its jaws, are first fully ignited and then fused, with splendid phenomena, while at the same time its intensity is so low that hardly the least visible spark can be made to pass by it through poles of carbon.

The magnetic effects attributed by Hare to his Calorimotor have since been shown by Henry to be attainable, as is now well known, from a single cell if combined with suitable conductors.

*Silliman's Journal (1) V. 3, p. 105.

EXPLANATION OF THE CALORIMOTOR.

A a, Fig. 1st, two cubical vessels, 20 inches square, inside. b b b b a frame of wood containing 20 sheets of copper, and 20 sheets of zinc, alternating with each other, and about half an inch apart. TTtt masses of ten cast over the protruding edges of the sheets which are to communicate with each other. Figure 2, represents the mode in which the junction between the various sheets and tin masses is effected. Between the letters zz the zinc only is in contact with the tin masses. Between c c the copper alone touches. It may be observed, that, at the back of the frame, ten sheets of copper between cc, and ten sheets of zinc between z z, are made to communicate, by a common mass of tin extending the whole length of the frame, between T T: but in front, as in Fig. 1, there is an interstice between the mass of tin connecting the ten copper sheets, and that connecting the ten zinc sheets. The screw forceps, appertaining to each of the tin masses, may be seen on either side of the interstice: and likewise a wire for ignition held between them. The application of the rope, pulley and weights, is obvious. The swivel at S permits the frame to be swung round and lowered into water in the vessel a, to wash off the acid, which, after immersion in the other vessel, might continue to act on the sheets, encrusting them with oxide. Between p p there is a wooden partition which is not necessary, though it may be beneficial.

EXPLANATION OF FIGURE 3 AND FIGURE 4.

Fig. 3, represents an apparatus analogous to a Couronne des Tasses, but reduced to a form no less compact than that of the trough; hollow parallelopipeds of glass are substituted for tumblers or cells. The plates are suspended to bars counterpoised like window sashes.

The apparatus of 300 pairs, alluded to in the first memoir on the Galvanic Deflagrator, was of the construction here represented. I have since arranged the glass parallelopipeds, in a trough, by the partial rotation of which they are filled and emptied.

In the philosophy of chemistry, Hare distinguished himself for the zeal and logical acumen with which he combated what he conceived to be the errors of the salt radical theory. He was ready at all times to engage in controversy upon any point of theory where he conceived there was an error latent. No one can review the numerous letters which he has addressed to the Senior Editor of this Journal (Silliman), to Berzelius, to Liebig and to Faraday, and published in this Journal, without perceiving that he was no ordinary antagonist.

In his family and among his friends Hare was very kind, and his feelings were generous, amiable and genial, although occasionally, his manner was abrupt—from absence of mind occasioned by his habitual abstraction and absorption in thought; his mind was ever active, and conversation would sometimes seem to awaken him from an intellectual reverie. He had high colloquial powers, but to give them full effect, it was necessary that they should be roused by a great and interesting subject, and especially if it assumed an antagonistic form. He would then discourse with commanding ability, and his hearers were generally as willing to listen as he was to speak.

He was a man of unbending rectitude, and a faithful friend both in prosperity and adversity.

Silliman's great joy was to work in Hare's company. And these two men, happy in one another's society, labored to advance their favorite science in this country. Their efforts were most successful. Their friendship formed in

early life continued. This is evident from the following letter:

Philadelphia, April 28, 1853.

My dear Silliman:

I thank you for your kind letter and am happy to hear upon the whole such good tidings of your own health and the prosperity of your progeny. You are much beyond me in the number of your tribe—the tribe of our modern Benjamin, one of the patriarchs of science if not of scriptural religion. It will give me much pleasure to make my abode with you this summer for as much time as can be spared from Family engagements. Time has passed as quickly with me during the last half year as at any period of my life. It fairly "gallops with-all" as it is by one of Shakespeare's wits represented as doing with a convict going to the gallows and from the very opposite reason. I wish the time longer in order to get through enterprises in given time; valuing it much it seems to be spent too quickly.

Though I cannot count many grandchildren I am very well satisfied with those which I have. I am devotedly attached to them and they seem to be fond of me, and if they are not all they might be they are as amicable and intelligent in my eyes as if they were really among the best.

I have been making an instrument to show how the wind would blow in a travelling whirlwind could such a storm exist. My instrument or that at first contrived performed so well as to lead me to hope to make one very near perfection but although I have got nearer to this attainment it seems as if in all human Efforts they bear the same relation to perfection as the hyperbola to the Asymptote. We may approach perfection perpetually but the result becomes at each approximation more difficult and as in dividing circulate we always find a remainder.

After I had succeeded in making many beautiful curves I found that it would be more expensive to transfer them to copper plate by any process than to engrave them on copper originally. Latterly I have been making an instrument for this purpose.

By some recent sales I have increased my income so that I no longer feel the loss as I did on first leaving my Professorship. The change this has induced has occupied me much of late. We are all in pretty good health.

I shall leave Mrs. Hare to answer for herself as respects the kind invitation of Mrs. Silliman and yourself.

Faithfully yours,

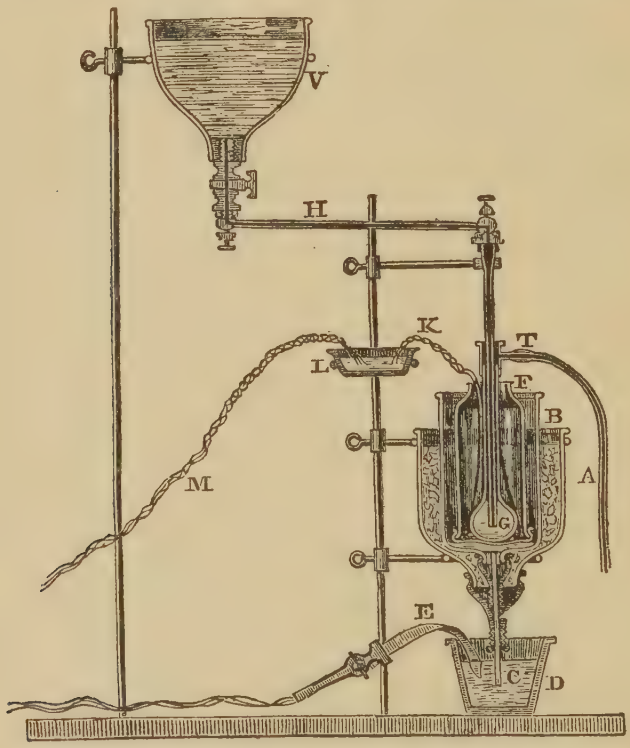
ROBERT HARE.

Robert Hare was interested in Galvanism, and watched the advances that were being made with the help of galvanic currents with deepest interest. In one of his papers he writes that he had observed Sir Humphrey Davy, in speaking of the isolation of the alkaline earth metals, include calcium in the number. Hare, however, was not altogether satisfied with Davy's results, and after considerable experimentation, presented (1835) a description of the course pursued by him in the isolation of the alkaline earth metals:

A DESCRIPTION OF THE APPARATUS AND PROCESS FOR OBTAINING AMALGAMS OF CALCIUM, BARIUM, AND STRONTIUM, FROM SATURATED SOLUTIONS OF THEIR CHLORIDES, BY EXPOSURE TO THE VOLTAIC CIRCUIT IN CONTACT WITH MERCURY, THIS METAL BEING THE CATHODE, AND A COIL OF PLATINA WIRE THE ANODE.

Two bell glasses, A and B, with perforated necks, were inverted and placed one within the other, so that

between them there was an interstice of half an inch, which was filled with a freezing mixture. Concentrically within B, a third similar bell, F, was placed, including a glass flask, of which the stem extended vertically through



HARE'S APPARATUS FOR OBTAINING AMALGAMS, ETC.

the neck of F. From a vessel, V, with a cock intervening, a tube, luted to the orifice of the flask, extended to the bottom of it, so as to convey thither from V, a current of ice water, which, after refrigerating the bulk of the flask, could escape through the nozzle projecting horizontally from the neck, F. The mercury in the capsule, D, communicates

through the rod with the negative poles of one or more deflagrators: the capsule, L, in like manner, with the corresponding negative poles.

A rod of platina reaches from some mercury in the capsule, D, through the necks of the bells O and B, into a stratum of mercury, resting upon the shoulder of the bell glass, B, so as to be about a quarter of an inch beneath the flask. Several circumvolutions of platina wire, in the form of a flat spiral, were interposed between the mercury and the bottom of the flask. The recurved ends of this wire were made to reach into the mercury in the capsule, L. Over the mouth of the bell, F, after the introduction of the flask and spiral, some bed-ticking was tied, so as to prevent contact between the platina and mercury, and to check, as much as possible, any reunion between the radical taken up by the one, and the chlorine liberated by the other. Into the bell, F, a saturated solution of the chloride to be decomposed, was poured, and some coarsely powdered crystals of the same compound added. Of course the solution, by penetrating the ticking, came into contact with the mercury.

ELECTROLYTIC PROCESS.

The peculiar mechanism of my apparatus, by which, in ten seconds, the acid may be thrown on or off of the plates, enables the operator within that time, after the poles are suitably arranged, to put either or both of the deflagrators in operation, or to suspend the action of either or both. This mode of completing or breaking the circuit, gives a great advantage in deflagrating wires; or in the processes wherein dry cyanides, phosphurets, or carburets, are to be exposed to voltaic action in vacuo, or in hydrogen. It enables us to arrange every part of the apparatus so as to produce the best effect upon the body to be acted upon, and then to cause a discharge of the highest intensity, of which

the series is capable, by subjecting all the plates at once to the acid, previously lying inactive in the adjoining trough.

In the case in point, where a chloride was to be decomposed, the deflagrators could be made to act through the same electrodes, either simultaneously or alternately. Of these facilities I thus availed myself.

Having supplied each deflagrator with a charge of diluted acid, of one-fourth of the usual strength, I began with No. 1, and at the end of five minutes, superseded it by putting No. 2 into operation. Meanwhile, having added to No. 1 as much more acid as at first, at the end of the second five minutes I superseded No. 2 by No. 1; and in like manner again superseded No. 1 by No. 2. Having thus continued the alternate action of the deflagrators, for about twenty minutes, both were made to act upon the electrodes simultaneously, the balance of acid requisite to complete the charge, having been previously added.

By these means the reaction was rendered more equable than it could become in operating with one series more highly charged. Although, under such circumstances, the reaction may at the outset be sufficiently powerful to produce ignition, as I have often observed, after fifteen or twenty minutes, it may become too feeble in electrolyzing power, to render the continuance of the powers in the slightest degree serviceable. Agreeably to my experience, as the ratio of the calcium to the mercury increases, the amalgam formed becomes so much more electro-positive, as to balance the electro-negative influence of the voltaic current. After reacting with one series of two hundred pairs, of one hundred square inches each, for twenty minutes, I have found the proportion of calcium to be only one six-hundredth of the amalgamated mass obtained.

In this lies the great difficulty of obtaining any available quantity of the radicals of the alkaline earths by electroly-

zation, especially in the case of calcium. It is easy by a series of only fifty pairs, to producce an amalgam with that metal, which, when exposed, will become covered with a pulverulent mixture of lime and mercury, but in such case the quantity of calcium taken up by the mercury, when estimated by the resulting oxide, will be found almost too small to be appreciated by weighing. To increase the quantity of calcium to an available extent, I have found extremely difficult, since, as the process proceeds, the chemical affinity becomes more active, while the electrolyzing power becomes more feeble.

That a change should be effected in mercury, giving to it the characteristics of an amalgam, by the addition of a sixth-hundredth part of its weight, cannot be deemed difficult to believe, when it is recollected that Davy found, that when, by amalgamation with ammonium, a globule of mercury had expanded to two hundred times its previous bulk, it had augmented only one twelve thousandths of its previous weight.

As the affinity between chlorine and the radicals of the alkaline earths increases in strength with the temperature, and as heat is evolved in proportion to the energy of the voltaic action, the disposition of the elements separated by electrolyzation to reunite is augmented in this way. Hence the necessity of refrigeration.

The best index of the success of this process, is the evolution of chlorine; since, in proportion to the quantity of this principle extricated at the anode, must be the quantity of calcium separated at the cathode. During my most successful operations, chlorine was evolved so copiously, as to tinge the cavity of the innermost bell with its well known hue. Hence, when the evolution of chlorine ceases to be quite perceptible, the amalgam should be extricated from the apparatus, and separated by a funnel and the finger from the solution of chloride, and subjected to distillation forthwith.

DISTILLATORY APPARATUS AND PROCESS.

The amalgam, to the extent of an ounce measure, was introduced into an iron crucible; of this crucible a section is here represented, which was forthwith closed by a capsule seated in a rabbet or groove made on purpose to

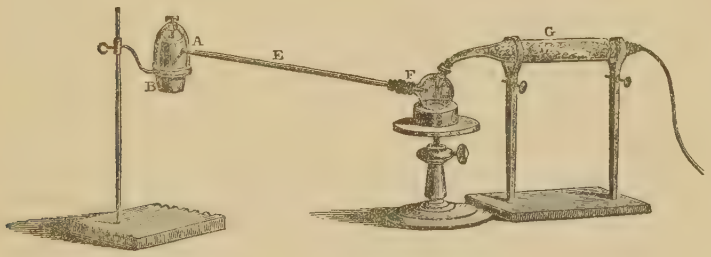
A Section of Hare's Amalgam Crucible.

receive it. The capsule being supplied with about half a dram of caoutchouchine, was then covered by the lid. In the next place, by means of a movable handle, or bail, or wire, so constructed as to be easily attached, the crucible was transferred to the interior of the body of the alembic. Into the cavity thus occupied, about a dram measure of naphtha was poured. The canopy, A, and body of the alembic, B, were then joined, as represented in the figure (page 199), with the aid of a luting of clay and borax, between the grooved juncture, and the pressure of the gallows screw provided for that purpose.

A communication was made, between the alembic and a small tubulated glass receiver F, by means of an iron tube, E, thirty inches long, and a quarter in bore. The tubulure of the receiver received the tapering end of an adopter, G, which communicated with a reservoir of hydrogen, by means of a flexible lead pipe. The length of the tube prevented the alembic or receiver from being subjected to the agitation which results from the condensation of the mercurial vapour. Before closing the juncture completely, all the air of the alembic was expelled by a current

of hydrogen, desiccated in its passage by a mingled mass of chloride of calcium and quicklime, contained in the adopter. By keeping up the communication with the reservoir, while the gas within it was subjected to a column of about an inch or two of water, the pressure within the alembic being greater than without, there could be no access of atmospheric oxygen.

The bottom of the alembic was protected by a stout capsule of iron (a cast iron mortar for instance). The next



HARE'S DISTILLATORY APPARATUS.

step was to surround it with ignited charcoal, in a chauffer or small furnace; taking care to cause the heat to be the greatest at the upper part. By these means, and the protection afforded by the mortar, the ebullition of the mercury may be restricted to the part of its mass nearest to the upper surface. Without this precaution this metal is liable to be thrown into a state of explosive vaporization, by which it is driven out of the crucible, carrying with it any other metal with which it may be united.

On the first application of the fire, the caoutchouchine was distilled into the receiver. Next followed the naphtha from the body of the alembic. Lastly, the mercury of the amalgam distilled, the last portions requiring a bright red heat, in consequence of the affinity between the metal and the alkaline radical.

After the distillation was finished, the apparatus having

been well refrigerated, the alembic was opened, and the crucible removed. As soon as the lid was taken off, some naphtha was poured between the rim of the capsule and sides of the crucible, so as to reach the metal below.

PROPERTIES OF THE METALS OBTAINED BY THE PROCESSES ABOVE MENTIONED.

Although not appertaining to galvanism or voltaic electricity, it may be expedient here, after describing the apparatus and process, to say something of the products obtained.

When the heat was sufficient to expel all the mercury, the metal was found adhering to the bottom of the crucible in a crust, which required an edge tool, to detach it, though no incorporation of the iron with it appeared to have taken place. When in distilling calcium, a crucible of platina was employed, a portion of this metal was found to have united with some of the calcium, being detached therewith in the form of a bright metallic scale.

In consequence of their susceptibility of oxidation, and of union with the elements of naphtha, the metals obtained as above mentioned were devoid of metallic lustre until their surfaces were removed by a file or burnisher. Either was rapidly oxidized in water, or in any liquid containing it; and afterwards, with tests, gave the appropriate proofs of its presence. They all sank in sulphuric acid; were all brittle, and fixed, and for fusion, required at least a good red heat. After being kept in naphtha, their effervescence with water is, on the first immersion, much less active. Under such circumstances, they react at first more vivaciously with hydric ether than with water, or even chlorohydric acid; because in these liquids a resinous covering, derived from the naphtha, is not soluble, while to the ether it yields readily.

By means of solid carbonic acid, obtained by Mitchill's

modification of Thilorier's process, I froze an ounce measure of the amalgam of calcium, hoping to effect a partial mechanical separation of the mercury, by straining through leather, as in the case of other amalgams. The result, however, did not justify my hopes, as both metals were expelled through the pores of the leather simultaneously, the calcium forming forthwith a pulverulent oxide, intermingled with and discoloured by mercury, in a state of extreme division.

By the same means I froze a mass of the amalgam of ammonium, as large as the palm of my hand, so as to be quite hard, tenacious and brittle. The mass floated upon the mercury of my mercurial pneumatic cistern, and gradually liquefied, while its volatile ingredients escaped.

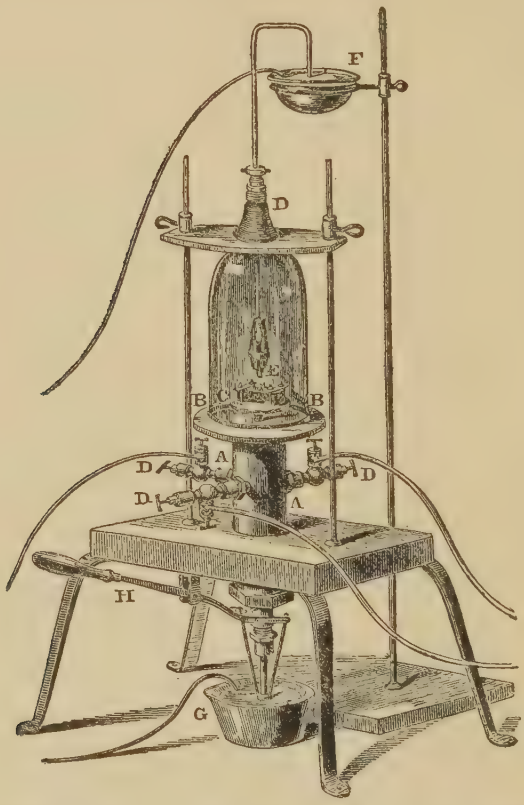
When the freezing of the amalgam was expedited by the addition of hydric ether, the resulting solid effervesced in water, evolving etherial fumes. This seems to show that a portion of this ether may be incorporated with ammonium and mercury, without depriving the aggregate thus formed of the characteristics of a metallic alloy.

The first electric furnace ever used was probably constructed and employed by Hare. This is apparent from the following communication:

DESCRIPTION OF AN APPARATUS FOR DEFLAGRATING CARBURETS, OR CYANIDES, IN VACUO, OR IN AN ATMOSPHERE OF HYDROGEN, BETWEEN ELECTRODES OF CHARCOAL, WITH AN ACCOUNT OF RESULTS OBTAINED BY THESE, AND BY OTHER MEANS; ESPECIALLY THE ISOLATION OF CALCIUM.

Upon a hollow cylinder of brass, A A, an extra pump plate, B B, is supported. The cylinder is furnished with three valve cocks, D D D, and terminates at the bottom in a stuffing-box, through which a copper rod slides so as

to reach above the level of the air pump plate. The end of the rod supports a small horizontal platform of sheet brass, which receives four upright screws. These, by pressure on



Hare's Apparatus for Deflagrating Carburets.

brass bars, extending from one to the other, are competent to secure upon the platform a parallelopiped of charcoal. Upon the air pump plate a glass bell is supported, and so fitted to it, by grinding, as to be air tight. The otherwise open neck of the bell is also closed air tight by tying about it a caoutchouc bag, of which the lower part has

been cut off, while into the neck a stuffing-box has been secured air tight. Through the last mentioned stuffing-box, a second rod passes, and terminates within the bell in a kind of forceps, for holding an inverted cone of charcoal, E.

The upper end of this sliding rod is so recurved as to enter some mercury in a capsule, F. By these means, and the elasticity of the caoutchouc bag, this rod has, to the requisite extent, perfect freedom of motion.

The lower rod descends into a capsule of mercury, G, being in consequence capable of a vertical motion, without breaking contact with the mercury. It is moved by the aid of a lever H, terminating in a stirrup, working upon pivots.

Of course the capsules may be made to communicate severally with the poles of one or more deflagrators. The substance to be deflagrated is placed upon the charcoal forming the lower electrode, being afterwards covered by the bell as represented in the figure. By means of the valve cocks, a communication is made with a barometer gage (like that described in my Compendium, page 77), also with an air pump, and with a large self-regulating reservoir of hydrogen. The air being removed by the pump, a portion of hydrogen is admitted, and then withdrawn. This is repeated, and then the bell glass, after as complete exhaustion as the performance of the pump will render practicable, is prepared for deflagration in vacuo. But if preferred, evidently hydrogen or any other gas, may be introduced from any convenient source, by a due communication through flexible leaden pipes and valve cocks.

Having described the apparatus, I will give an account of some experiments performed with its assistance, which, if they could have illuminated science as they did my lecture room, would have immortalized the operator. But alas we may be dazzled and almost blinded by the light produced by the hydro-oxygen blowpipe or voltaic ignition, without being enabled to remove the darkness which hides the mysteries of nature from our intellectual vision.

I hope, nevertheless, that some of the results attained, may not be unworthy of attention, and that as a new mode of employing the voltaic circuit, my apparatus and mode of manipulation will be interesting to chemists.

An equivalent of quicklime, made with great care from pure crystallized spar, was well mingled by trituration with an equivalent and a half of bicyanide of mercury, and was then enclosed within a covered porcelain crucible. The crucible being included within an iron alembic (see p. 199), the whole was exposed to a heat approaching to redness. In two experiments the residual mass had such a weight as would result from the union of an equivalent of cyanogen with an equivalent of calcium.

A similar mixture being made, and in like manner enclosed in the crucible and alembic, it was subjected to a white heat. The apparatus being refrigerated, the residual mass was transferred to a dry glass phial with a ground stopper.

A portion of the compound, thus obtained and preserved, was placed upon the parallelopiped of charcoal, which was made to form the cathode of two deflagrators of 100 pairs each like that described on page 196 acting together as one series.

In the next place the cavity of the bell glass was filled with hydrogen by the process already described and the cone of charcoal being so connected with the positive end of the series as to be prepared to perform the office of an anode was brought into contact with the compound to be deflagrated. These arrangements being accomplished and the circuit completed by throwing the acid upon the plates, the most intense ignition took place.

The compound proved to be an excellent conductor, and during its deflagration emitted a most beautiful purple light, which was too vivid for more than a transient endurance, by an eye unprotected by deep coloured glass. After the compound was adjudged to be sufficiently deflagrated,

and time had been allowed for refrigeration, on lifting the receiver, minute masses were found upon the coal, which had a metallic character, and which, when moistened, produced an effluvium, of which the smell was like that which had been observed to be generated by the silicuret of potassium.

Similar results had been attained by the deflagration, in a like manner of a compound procured by passing cyanogen over quicklime enclosed in a porcelain tube heated to incandescence.

Phosphuret of calcium, after being intensely heated without access of air, was found to be an excellent conductor of the voltaic current, evolved from the apparatus above mentioned. Hence it was thought expedient to expose it in the circuit of the deflagrator, both in an atmosphere of hydrogen, and in vacuo. The volatilization of phosphorus was so copious, as to coat throughout the inner surface of the bell glass with an opake film, in colour resembling that of the oxide of phosphorus, generated by exposing this substance under hot water, to a current of oxygen.

The phosphuret at first contracted in bulk, and finally was for the most part volatilized. On the surface of the charcoal adjoining the cavity in which the phosphuret had been deflagrated, there was a light pulverulent matter, which, thrown into water, effervesced, and when rubbed upon a porcelain tile, appeared to contain metallic spangles which were oxydized by the subsequent exposure to atmospheric oxygen.

Charles Doremus (Transactions of the American Electrochemical Society, 13, p. 358) writes: "Hare obtained, despite the disadvantages of a current from a primary battery, calcium carbide, phosphorus, graphite, and metallic calcium, and there are just reasons for regarding him as the earliest American scientific experimenter and discoverer in electro-chemistry."

CHAPTER IX

SILLIMAN, the elder, came a second time to Philadelphia (Nov. 5, 1803, to March 25, 1804) to study science, because that city offered more opportunities for such study, at that time, than any other American city. He has left a record of that year in his diary:

There was little to distinguish this from the preceding winter. I attended, as before, the course of chemistry and anatomy, and resumed my private labours with Robert Hare. The familiarity which I had acquired in the preceding year with men and things, enabled me to derive additional advantage, and made me feel more at home. My circle of acquaintance was more extended quite as much as was consistent with my studies. I was admitted hospitably or socially to some of the most estimable families,—that of Judge Wilson, son of him of the Revolution; to Bishop White's, Dr. Strong's, Col. Biddle's, where there were beautiful daughters (afterwards Mrs. Dr. Chapman and Mrs. Cadwallader). I have mentioned the Wistars, Bainbridges and Greens. At Judge Peter's, also, I was acquainted, and at Mrs. Bradford's. I visited also the public institutions,—the Hospital, the Mint, the Navy Yard, the Water Works, the libraries, manufactories, etc. Philadelphia had then seventy-five or eighty thousand inhabitants; now it has more than half a million. The present beautiful Washington Square was a Potter's Field, and all was country between it and the Hospital. About this time I was elected a member of the Philosophical Society founded by

Benjamin Silliman

Franklin, and of course had free access to its library, and to its very intelligent and kind librarian, Mr. John Vaughan, a man of large benevolence. I continued the writing of my lectures, and began to collect apparatus, although on a humble scale.

In March, 1804, after passing a few days at Princeton, I returned to New Haven, and devoted my time to writing lectures.

In 1805, Silliman (1779-1864) went to Europe, remaining abroad a little more than a year. He attended lectures in London and in Edinburgh, meeting many of the foremost scientific men of the period. In 1811 he instituted and carried out experiments with Robert Hare's blow-pipe, in which he succeeded in melting lime, magnesia, rock crystal, gun flint, corundum gems and a long list of the most refractory minerals, a greater part of which had never been melted before.*

On receiving intelligence of Sir Humphry Davy's discovery of the metallic bases of the alkalies, he completely repeated his experiments and obtained, probably for the first time in the United States, the metals potassium and sodium'' (p. 129). Silliman was the first to fuse carbon in the voltaic arc (p. 184). In 1819, he began the publication of the *American Journal of Science*, popularly known as *Silliman's Journal*. In 1829, he edited an edition of Bakewell's Geology; in 1820, he published an elaborate ''Treatise on General Chemistry'' in two volumes. It lays no

* He wrote: ''The fusion and combustion and complete dissipation of platinum, gold, silver, nickel, cobalt and most of the metals, and the fusion of the principal earths and of their more refractory compounds, by the use of Hare's compound blow-pipe, have been the *familiar and easy class experiments* of every course of chemistry in Yale College for these eight years.''

claim to originality in the treatment of the subject. From the results of his own laboratory and from his much reading, he gathered all the known facts and laws of the science and moulded them in a form which he deemed most convenient for instruction.

His special field was the diffusion of science, and his special gifts and acquirements made him one of the most popular scientific lecturers in the country. Without being profound or original, he selected from the great storehouse of knowledge, all familiar to him, so judicially, and threw such an enchantment round his theme that all felt a kindling of enthusiasm as they listened. They drank in the doctrines of latent heat and chemical equivalents, saw through all the forms and laws of crystallization, and plainly read in minerals and fossils and rocks of the fields the geological eras which stretched back into the immeasurable past where no human eye ever saw.

The utility of science in its broadest sense was always uppermost in his mind. In his several books and papers, he aims at the accomplishments of usefulness.

When the Chemical Society of Philadelphia ceased to exist, there was an interim in which no gatherings of chemists were held. But, in 1811, many, feeling the need of the influence of a society devoted to their special science, organized the Columbian Chemical Society of Philadelphia. The title page and a number of pages from the first volume of proceedings are herewith submitted:

MEMOIRS

OF THE

COLUMBIAN CHEMICAL SOCIETY,

OF PHILADELPHIA

VOLUME I

PUBLISHED BY ISAAC PEIRCE,

No. 3, South Fourth Street.

1813.

PREFACE

IN presenting to the public the first fruits of the Columbian Chemical Society, it may not be amiss, to give a short history of its rise and progress.

The Columbian Chemical Society was founded in the month of August 1811, by a number of persons desirous of cultivating chemical science, and promoting the state of philosophical enquiry. And although the number of members has not increased rapidly; yet, from the zeal manifested by the few who compose the Society, in investigating scientific topics, there is every reason to believe, that the period is not far distant, when this institution will rank high in respectability.

From the nature of the subject to be investigated, it followed, that essays would be frequently offered by its members. Hence arose that multiplicity of dissertations from which this selection has been made. The appearance, however, of some papers, whose relation to chemistry, is scarcely, if at all perceptible, prompts us to make one remark. One of the articles of the constitution expressly declares, that communications may be on subjects either chemical or philosophical.

The contents of the volume, are chiefly original, partly speculative, and partly practical. The great variety of topics investigated, the novelty of the essays, will, it is hoped, render the whole, somewhat interesting. In addition to the papers of the Society, it has been deemed proper to publish the Constitution, together with a list of officers, members, &c. &c.

CONTENTS

Page

Remarks on the Phlogistic and Antiphlogistic Systems of Chemistry. By Thomas D. Mitchell, M. D..... 5

An Inquiry into what circumstances will warrant us justly to reckon a substance a principle of a common property of any set of bodies. By Franklin Bache 15

On the prognostic signs of the weather. By James Cutbush 26

Experiments and observations on the effect of light on vegetables and upon the physiology of leaves. By John Manners, M. D..... 47

Speculations on lime. By Joel B. Sutherland, M. D. 58

Remarks on heat. By Thomas D. Mitchell, M. D.... 63

On the oxyacetite of iron as a test or re-agent for the discovery of Arsenic. By James Cutbush..... 70

Thoughts on the principle of excitability. By George Ferdinand Lehman 75

Analysis of a Mineral spring, at the Willow Grove, Montgomery County, Pennsylvania. By John Manners, M. D., and Thomas D. Mitchell, M. D..... 93

An Inquiry whether Mr. Berthollet was warranted, from certain experiments, in framing the law of chemical affinity, "that is directly proportional to the quantity of matter." By Franklin Bache..... 98

On muriatic and oxy-muriatic acids, combustion, &c. By Thomas D. Mitchell, M. D..... 102

On the production of Sulphuretted Hydrogen by the action of Black Sulphuric Acid, diluted with Water, on Iron Nails. By John Manners, M. D..... 118

On the emission of oxygen gas by plants. By George

Ferdinand Lehman 121
Analysis of Malachite, or green carbonate of copper of Perkioming Pennsylvania. By Thomas D. Mitchell, M. D. 125
Thoughts on the expediency of changing parts of the chemical Nomenclature. By Franklin Bache..... 127
Remarks on Putrefaction. By Thomas D. Mitchell, M. D. 135
A few remarks upon the nature of the nervous influence. By Joel B. Sutherland, M. D..... 145
Chemical view of secretion. By Thomas D. Mitchell, M. D. 158
Observations upon the effects of various gases upon the living Animal Body. By Edward Brux, of France 158
Analysis of professor Coxe's essay on combustion and Acidification 174
Experiments and observations on putrefaction by John Manners, M. D..... 190
Observations on the formation of muriate of potash in the process of preparing the hyperoxymuriate of potash by William Hembel, jun. Esq..... 202
Analysis of the Bordentown (N. J.) spring. By Samuel F. Earl..... 205
Report of the committee to whom was referred the analysis of certain ores, presented through the medium of Thomas Brientnall, Esq. to the Columbian Chemical Society 208
Remarks on the atmosphere, read before the Columbian Chemical Society, by Thomas D. Mitchell, M. D. 211
A new method of mounting Woulff's Apparatus in which the tubes of safety are superceded, by William Hembell, jun. Esq. Communicated to the society by John Manners, M. D..... 218

OFFICERS OF THE COLUMBIAN CHEMICAL SOCIETY

Hon. Thomas Jefferson, Esq. Patron.
James Cutbush, Esq. Profess. Nat. Philos. Chemistry and Mineralogy in St. John's College. President.
George F. Lehman, Franklin Bache } Vice Presidents.
John Barnes, M. D., John Lynn, M. D., Charles Edwards } Corresponding Committee.
John C. Heberton, Secretary.
James J. Hamm, Treasurer.
John R. Barnhill, Orator.

Junior Members.

Abraham Armstrong, of Santa Cruz.
Thomas F. Breintnall, Esq. of Philadelphia.
Francis H. Brognard, of New Jersey.
Edward Brux, of France.
D. Burwell, of Virginia.
Samuel F. Earl, of Philadelphia.
Lewis Gebbard, of New York.
George Grey, of Philadelphia.
Thomas C. Hall, of Maryland.
Henry Hawkins, of Maryland.
Henry Neill, of New Jersey.
T. W. Robertson, of South Carolina.
Wm. Shippen, of Philadelphia.

Honorary Members.

M. Adet, of France.
F. Aigster, M. D., Lecturer on Chemistry at Pittsburg.

Mr. Allen, of Great Britain.
Right Hon. Sir Jos. Banks, Brt. K. B. P. R. S.
Benjamin Smith Barton, M. D. Profess. of Mat. Med. Nat. Hist. & Bot. in Univer. of Pennsyl.
M. Berthollet, of France.
A. Bruce, M. D. Profess. of Mineralogy, Columbia College, New York.
Chas. Caldwell, M. D. Lecturer on the Inst. and Practice of Medicine, of Philadelphia.
M. Chaptal, of France.
J. Cloud Esq. of Philadelphia.
Samuel Conover, M. D. of Philadelphia.
Hon. Thos. Cooper Esq. Profess. of Chemistry, Mineralogy and Geology, in Carlisle College.
Jno. Redman Coxe, M. D. Profess. of Chemistry in the University of Pennsylvania.
Edward Cutbush, M. D. Surgeon in the U. S. Navy.
Jno. Dalton Esq. F. R. S. E. of Manchester.
Sir Humphry Davy, Knt. LL. D. Sec. R. S. London.
Jno. Davy, Esq. London
Elisha DeButts, M. D. Profess. of Chemistry in the College of Maryland.
J. A. DeLuc, F. R. S. England.
M. Deyeux, France.
Benj. DeWitt Profess. of the Institutes of Medicine and Lecturer of Chemistry, N. York.
John Syng Dorsey, M. D. Adjunct Profess. of Surgery, Univer. of Pennsylvania.
Jno. Griscom, Lecturer on Chemistry, N. York.
Robert Hare, Profess. Nat. Philos, Univer. Penn.
C. Hatchett, Esq. F. R. S. London.
Abbe Hauy, France.
Wm. Hembell, Esq. Philadelphia.
Wm. Henry, M. D. F. R. S. Manchester.
Wm. Herschell, L. L. D., F. R. S. England.
T. T. Hewson, M. D. Philadelphia.

Jno. Hope, M. D. Profess. Chemistry Edinburg.
David Hosack, M. D. Profess. of the Theory and Practice of Medicine and of Clinical Medicine in the University of New York.
Henry Jackson, M. D. Profess. of Chemistry, Athens College, Georgia.
M. Boillon Lagrange, France.
M. Lussac, France.
His Excel. James Madison, Esq. LL. D. President of the United States of America.
John Manners, M. D. F. A. N. S. of Philadelphia.
Jno. McLean, Profess. of Chem. Nat. Philos. Princeton College.
Hon. Saml. L. Mitchill, M. D., F. R. S. E. Profess. Nat. Hist. Botany, New York.
Thos. D. Mitchell, M. D. F. A. N. S. of Philadelphia.
M. Monge, of France.
M. Guyton Morveau, of France.
Jno. Murray, Esq. Lecturer on Chemistry, Materia Medica and Pharmacy, Edinburg.
Wm. Nicholson, Esq. London.
Jno. C. Osborne, M. D. Profess. Inst. and Practice of Medicine, Columbia College, N. York.
M. Parmentier, of France.
Joseph Parish, M. D. of Philadelphia.
Robert Patterson, A. M. Profess. Mathematics, & Lecturer on Nat. Philos. University of Penn.
G. Pearson, M. D., F. R. S. of London.
W. H. Pepys, Esq. F. R. S., England.
M. Pelletier of France.
Nathaniel Potter, M. D. Profess. of the Theo. and Practice of Medicine, University of Maryland.
M. Proust, Professor Chemistry, Madrid.
Benjamin Rush, M. D. Profess. of Inst. and Practice of Medicine, University of Pennsylvania.
M. Seguin, France.

Hon. Adam Seybert, M. D. of Philadelphia.
David Shepperd, M. D. of New Jersey.
Benjamin Silliman, Professor of Chemistry, Yale College.
John S. Stringham, M. D. Professor of Chemistry, N. York.
Joel B. Sutherland, M. D. F. A. N. S. Philadel.
M. Thenard, France.
Thomas Thomson, M. D. F. R. S. E. Lecturer on Chemistry, Edinburg.
Alexander Tilloch, Esq. M. R. S. et F. S. At London.
Jared Troust, M. D. Lecturer on Mineralogy in the Academy of Natural Sciences.
M. Vauquelin, France.
Lawrence Washington, Esq. Virginia.
Caspar Wistar, M. D. Profess. Anat. Univer. of Pennsylvania.
Chas. Wistar, Esq. of Philadelphia.
Wm. Hyde Wollaston, M. D. Sec. R. S. London.

It is said that two volumes of "Memoirs" were published by this Society, although the writer has seen but one after years of search. It is quite stimulating to observe that in this new country, young as it was, there should have been two such strong efforts made to establish societies of a National character, devoted particularly to chemistry.

CHAPTER X

FROM the preceding pages it is evident that, at the close of the eighteenth and beginning of the nineteenth century, creditable advances were being made in science in this western world. Many of the leading chemists were devoted to mineralogy. They discovered new methods of analysis and brought to light many interesting species.

In this group of students, occupying a most prominent position, was Archibald Bruce (1777-1818), the editor of the *American Mineralogical Journal of New York.* He had carefully analyzed zincite and written extensively upon the ores of titanium found in the United States. He will, perhaps, be longest remembered by the fact that he first called attention to the beautiful mineral which bears his own name—brucite. His collection of minerals possessed great value. In this connection, it may be noted that probably the first mineral analysis of any importance made in this country was that of the mineral chondrodite, which had been carried out by his assistant, Dr. Langstaff. Berzelius had failed in his analysis of the same mineral to note the fluorine content which was observed by Langstaff.

It is said that Thomas G. Clemson (1807-1888), educated in Chemistry at the School of Mines in Paris, was the first to announce the discovery of the diamond in the itacolumite of North Carolina. Other minerals were announced

by him at various times but his contributions to pure chemistry do not seem to be well known.

Lardner Vanuxem (1792-1848), one of that small group of Americans privileged to study at the School of Mines in Paris, in the early years of 1800, devoted himself on his return almost entirely to investigation in pure chemistry and in mineralogy. Some of his researches were conducted in conjunction with his friend, William H. Keating. Their contributions may be found in the "Proceedings of the Academy of Natural Sciences" (Phila.). He analyzed the phosphate of iron from New Jersey; the tabular spar from Willsborough, jeffersonite, zircon, marmolite, and other serpentines. He and Keating issued (1822) the "Mineralogy of Sussex County, N. J." For a period in his life he held the chair of Chemistry in the University of South Carolina, and later had charge of the geological survey of New York. His large and beautiful collection of minerals became, after his death, the property of the Masonic College at Clarksville, Tenn.

In the early journals devoted to science, the name of John Torrey (1798-1873) is frequently encountered. He occupied the chair of Chemistry at West Point, later, in the College of Physicians and Surgeons in New York, and afterward at Princeton. He became the head of the Chemical Department of the U. S. Assay Offices in New York. Most of his publications were printed on the pages of the *American Journal of Science,* where, in the year 1873, a sketch appeared of him and his scientific labors. In 1824, he issued a paper "On Yenite in the United States," the same year an "Account of the Columbite of Haddam"; in 1836, "Notes on American Minerals"; in 1825, "Vau-

quelinite in the United States''; in 1818, ''Vauquelinite in the United States''; in 1848, ''On Staurotide''; in 1820, ''On Siderographite''; in 1822, ''On an Ore of Zinc at Ancran''; in 1825, ''On West Point Minerals''; in 1839, ''On the Condensation of Carbonic, Sulphurous and Chlorochromic Acid Gases.''

Joseph Cloud (1770-1845), Assay Master of the United States, described in the ''Transactions of the American Philosophical Society,'' an alloy of palladium and gold from Brazil, which was followed up with a further study of the platinum metals, and it is said that he obtained rhodium.

Parker Cleaveland (1780-1858), professor at Bowdoin College, Maine, from 1803 to the time of his death, was an excellent chemist, although his reputation as a scientist was mainly due to his ''Elementary Treatise on Mineralogy,'' a most important contribution to American scientific literature.

The Erving Professorship at Harvard was occupied by John Gorham (1783-1829) from 1816 to 1827. He published two volumes on ''The Elements of Chemical Science.'' The introduction of this book is well worth perusal by chemists of the present day, because of its wide philosophical spirit. Silliman thought well of the publication and said it was unsurpassed by any with which he was acquainted. It was the first systematic treatise on Chemistry by an American. Gorham is further credited with ''An Analysis of Heavy Spar from Hatfield,'' ''Chemical Examination of Sugar,'' ''Chemical Analysis of Indian Corn,'' ''Indiogene,'' etc.

James Freeman Dana (1793-1827) (Dartmouth, 1820)

had been the assistant of Gorham at Harvard and studied Chemistry in the Laboratory of Accum, in London. His contributions were: "On a New Form of Electrical Battery," "Chemical Examination of the Berries of Myrica Cerifera," "The Effect of Vapor on Flame," "On the Existence of Cantharidin in the Potato Fly," "On the Theory of the Action of Nitrous Gas on Eudiometry." The alkaloid sanguinarine was discovered by him in the roots of the Sanguinaria Canadensis.

Samuel Luther Dana (1795-1868), living at the same time, was an acknowledged authority in technical chemistry. He was the inventor of the "American System" of bleaching. He made a study of manures and published a volume called "The Muck Manual for Farmers."

George T. Bowen (1803-1828), a student of the elder Silliman, as early as 1822 carried on investigations upon "The Electro-magnetic Effects of Hare's Calorimotor," and wrote "On a Mode of Preserving in a Permanent Form the Coloring Matter of the Purple Cabbage as a Test for Acids and Alkalies," as well as described the minerals scheelite, silicate of copper, nephrite, pyroxene-sahlite.

Bowen's successor in the chair of Chemistry at Nashville was Gerard Troost (1776-1850), a Hollander, educated at Amsterdam, Leyden and Paris. On his arrival in America in 1810, he settled in Philadelphia. He was one of the founders and first President of the Academy of Natural Sciences. He was a very regular contributor to the early volumes of the "Transactions" of the Society. Nearly all his papers deal with mineralogical subjects. In 1814, Dr. Troost established works for the production of alum at Cape Sable, Maryland, which was one of the earliest

chemical industries in the country and the first establishment for the manufacture of alum. The geological survey of Tennessee was instituted by Troost. He had an excellent knowledge of meteoric bodies, and made a large collection of the same, studying their chemical and physical properties. More than 14,000 minerals, which, of course, included his meteorites, were described by him. After his death his collection was turned over to the Public Library of Louisville.

In the University of North Carolina, Denison Olmsted (1791-1859) filled the chair of Chemistry (1817-1825). It is said that he made the first attempt in the United States to have a state geologically surveyed and published a report in 1824-25 that covered over 140 pages. On becoming professor at Yale he published a "Memoir on the State of Chemical Science" which may be read now with curious interest, as a record of the then existing state of philosophical opinion in our science. It was a sort of résumé of all that had been done in chemistry.

John Redman Coxe (1773-1864), of the Medical School of the University of Pennsylvania, was the associate of Thomas Cooper (p. 128) in the publication of "The Emporium of Arts and Science." In 1816 he issued a paper entitled a "Plan for Electric Telegraphy" which antedates any other American suggestion on this subject. He further wrote on "Phosphorus," "Observations on Crystallization," "On Lead Pipes," and "On the Preparation of Phosphuret of Lime."

Reference to the list of members of the Columbian Chemical Society will disclose the name of James S. Cutbush who was president, for a while, of the Society, as well

as Professor of Chemistry in the U. S. Military Academy at West Point, and before that time at St. John's College, Maryland. He wrote (1822) "On the Formation of Cyanogen in Some Chemical Processes Not Before Noticed." In this paper he tells of getting cyanogen from the action of nitric acid upon charcoal. He published, also, papers "On the Composition and Properties of the Chinese Fire and the So-Called Brilliant Fires" in the 7th volume of the *American Journal of Science,* and "On the Composition and Properties of Greek Fire." In 1813 he issued his "Philosophy of Experimental Chemistry" in two volumes.

The University of Virginia, founded about 1820, had as the first occupant of its chemical professorship (1824-42), Dr. J. P. Emmett (1799-1842), who published these papers: "On Iodide of Potassium as a Test for Arsenic," "Upon the Solvent and Oxidating Powers of Ammoniacal Salts," "Bromine and Iodine in Kanawha Salts," "On Formic Acid," and "On the Solidification of Raw Gypsum."

In New York, John Griscom (1774-1852), whom his contemporaries considered to be the best of teachers in chemistry, contributed abstracts of chemical papers to the *American Journal of Science,* but there are no records of his own personal investigations.

One of the contemporaries of Joseph Cloud, Dr. William J. McNevin (1763-1841), published "An Exposition of the Atomic Theory," which was received very favorably, and also published an edition of Brande's Chemistry. In addition he wrote on the "Decomposition of Potash," "Chemical Examination of the Waters of Schooley's Mountain," "On the Oxyacetate of Iron as a Test or Reagent for the Discovery of Arsenic" (1812).

At West Point during this period was W. W. Mather (1804-1859), probably best known as a geologist, as he had charge of the geological survey of the first district of New York State. He died while President of Ohio State University at Columbus, O. In the *American Journal of Science* for 1835 he contributed a memoir entitled "Contribution to Chemical Science" and, subsequently, "Chloride of Aluminium and Its Analysis," "Hydrated Chloride of Aluminium," "Crystallized Tin from Solution," "Georgia Gold," "Silver of Lane's Mine," "Iodide of Potassium or Iodo-platinate of Potassium," "Chloriodide of Platinum," "Crystallized Perchloride of Platinum," "Amalgam of Platinum," "Iodide of Mercury," "Solubility of Bitungstate of Ammonia," "Bisulphuret of Bismuth." It is an exceedingly interesting memoir. A portion of the contribution on chloride of aluminium relates to the atomic weight of aluminium. Indeed, this is probably the first paper on atomic weight work done in the United States. It is a creditable production and, when we consider the period in which it was carried out, 1835, it was probably the most original research in pure inorganic chemistry made by an American chemist up to that time. Many years elapsed before any other American published atomic weight determinations. The value Mather gave for aluminium is 27, deduced from the ratio between AgCl and $AlCl_3$, but the variations between the determinations are great. One need merely read the description of his mode of procedure to understand that it was very possible for him to have had wide differences between the individual results. There was no attempt made to obtain pure material, and no attention given to the defects of the methods of analysis.

The computations were to the third decimal place only. However, it was an effort in the right direction, and it is interesting.

Lewis C. Beck (1798-1853), another mineralogical chemist, prepared a "Mineralogy of the State of New York" as early as 1842. The analyses reported in the volume were all made by Beck. In 1827 he contributed "General Views on the Formation of Phosphuretted Hydrogen"; in 1828, "On the Nature of Bleaching and Disinfecting Compounds"; "On the Functions of Nitrogen in Respiration"; "On the Commercial Potashes of New York"; "On Wines and Other Fermented Liquors"; and "On Adulterations of Various Substances Used in Medicine and the Arts." He conducted a series of "Researches on the Bread Stuffs of the United States," which was published by the United States Patent Office at Washington. He was, further, the author of a book called "A Manual of Chemistry," published in 1831. This book passed through four editions.

The researches of J. W. Bailey (1811-1857), at West Point, were of such a character that many would not place him among the chemists of the country. These were principally "experiments with the aid of the microscope"; yet he published a test for nitric acid, a paper on "Double Cyanide and Iodide of Mercury," "A Test for Sulphure by Playfair's Nitro-Prusside." He was skillful in the use of the blowpipe and published a paper "On the Common Blowpipe." Nearly all of his contributions appeared in the *American Journal of Science.*

It would seem that Samuel Guthrie (1782-1848), of New York, discovered chloroform independently of Soubeiran,

Liebig, and Dumas. Hence the following quotation is quite apropos.

So early as 1796, an association of four Dutch chemists, who had already discovered the rich hydro-carbon gas, long known as heavy carburetted hydrogen gas, or olefiant gas, and now called ethylene or hydrogen-dicarbide (C_2H_4), studied the effects produced from mingling this hydrocarbon with an equal volume of chlorine gas over water. They saw that the volume of mixed gases rapidly diminished, with a notable elevation of temperature and the appearance of a dense oily-looking liquid, collecting on the sides of the bell-jar and the surface of the water, and quickly sinking to the bottom. Collecting this oily liquid and washing it clean of adhering chlorine, in alkaline water, and in pure water, it was found to be a new substance of a highly agreeable ethereal odor, and distinctly sweetish aromatic taste, neutral to tests, and nearly insoluble in water, to which, however, it imparted its taste and odor, but quite soluble in ether and alcohol. It was wholly unaffected by concentrated sulphuric acid even with the aid of heat. For many years its real constitution remained unknown, and it was shown only as one of the curiosities of the chemist's laboratory, under the name of "Oil of the Dutch Chemists"; the name olefiant gas having had its origin from the oil producing property, which this gas developed in its action with chlorine. Analysis has long since shown that this chlorine compound of the Dutch chemists is a simple union of one molecule of ethylene with two atoms of chlorine, and that it may properly be called the chloride of olefiant gas. I have been the more particular in noticing the discovery of this remarkable substance because it has acquired considerable notoriety from the fact that it was early and most naturally confounded with chloroform, to which, in sensible and physiological properties, it bears a remarkable resemblance. It

was long known as "Chloric Ether," a name which conveys a false meaning, since there is nothing in the chemical constitution of the body which in the least resembles the ethers.

In 1831 appeared the second volume of "Silliman's Elements of Chemistry," in the order of the lectures then given in Yale College, in which the Dutch liquid was spoken of in its physiological relations, with the remark that, "Its medical powers have not been ascertained, but from its constitution and properties it is highly probable that it would be an active diffusive stimulant."

This remark immediately attracted the attention of Dr. Samuel Guthrie, of Sackett's Harbour, New York, a man of an active and original mind, much devoted to practical chemistry, who at once conceived that he might obtain the so-called "chloric ether" in greater abundance and at a cheaper cost by distilling together alcohol and chloride of lime (bleaching powder). His success was remarkable, and he obtained the alcoholic solution (of chloroform) in great abundance, describing his process in a short article in *Silliman's Journal of Science* for January, 1832; and subsequently, in July of the same year, he states with more detail the precautions he adopted to obtain the product pure, and, especially, free from alcohol. It is remarkable that in his second paper he describes in full the method of testing the purity of the substance by agitation with concentrated sulphuric acid. There is no question that Dr. Guthrie was entirely original in his method of producing "chloric ether," as it was then called, and it is no abatement of his sagacity that he was not aware that, earlier in the same year in which he described his process, a French chemist, Mr. Soubeiran, had devised and described the same method in a memoir entitled, "*Researches on Some Combinations of Chlorine,*" which appeared in the *Ann. de Chimie et de Phys.* for Feb. 1831. Soubeiran called the product "a new ethereal liquid of a constitution unlike any before known to chemists," and also gives the name

chloric ether (ether chlorique). The term "chloric ether" had also been used by Dr. Thomson in 1820 to describe the oil of the Dutch Chemists. Soubeiran gave two analyses of this product, which, while they prove that the body is not the "Dutch liquid," failed to reveal its true constitution, which was first given by Dumas in 1834, in a memoir published by him in the same journal, and in this paper Dumas then gave to the new body the name by which it has ever since been known, chloroform.

Such, in brief, is the history of one of the most remarkable bodies ever discovered. While the "chloric ether" of Guthrie was a misnomer, the substance which he produced was chloroform, and the first use made of this agent in medical practice was at the suggestion of Prof. Silliman, to Dr. Eli Ives, formerly Professor of Theory and Practice in Yale in 1832. The case in which he employed it was one of asthma in an aged person, who was relieved of a severe paroxysm by its use "more suddenly than she had been in any previous illness of the kind." Thus the therapeutic history of chloroform had its commencement from the teachings and practice of the Yale Medical School.

The question of absolute priority of the discovery of chloroform may give it to the French chemist Soubeiran, but a committee of the Medico-Chirurgical Society, of Edinburg, have awarded to Dr. Guthrie the merit of having first published an account of its therapeutic effects as a diffusible stimulant in 1832. Chloroform may, therefore, fairly be claimed as an American discovery. Guthrie also experimented with a boldness and rashness in the preparation of fulminating compounds, of which he manufactured large quantities, of various and original constitution, for commercial purposes. His papers on this subject in vol. XXI. (1832) of the *American Journal of Science* disclose his power as an originator of new methods in chemistry. This is true, also, of his process for the rapid conversion of potato starch into sugar, printed in the same volume.

Another striking figure was Alexander Dallas Bache (1806-1867). Philadelphia was his birthplace. At fifteen years of age he was appointed a cadet in the Military Academy at West Point. Subsequently, he became a professor in the University of Pennsylvania. "Bache was a member of the Franklin Institute and also of the American Philosophical Society. He erected an observatory in the yard of his dwelling, in which, with the aid of his wife and his former pupil, John F. Frazer, he determined with accuracy, for the first time in this country, the periods of the daily variations of the magnetic needle. He was not destined to remain in his position in the University. Before he had become fairly settled in it and had renewed his familiarity with its duties, he was called in November, 1843, on the occasion of the death of Mr. Hassler, Superintendent of the U. S. Coast Survey, to fill the important sphere of public duty thus made vacant." He died in the 61st year of his age.

His papers were quite numerous. The first one, in 1829, was "On the Specific Heat of the Atoms of Bodies"; in 1830, "On the Inflammation of Phosphorus in a Partial Vacuum"; in 1832, "A Translation of Berzelius' Essay on Chemical Nomenclature." In molecular physics his "Inquiry in Relation to the Alleged Influence of Color on the Radiation of Non-luminous Heat" has become a classic. It is almost needless to say that he proved by it the fallacy of the notion, till then commonly received, that color did influence the radiation of non-luminous heat.

Numerous publications from William J. Taylor (1833-1864) appeared in the "Proceedings of the American

Philosophical Society." These relate, chiefly, to minerals, and there is every reason to believe that he was a competent analyst. In 1845, James D. Whelpley printed a rather interesting paper entitled "Idea of an Atom, Suggested by the Phenomena of Weight and Temperature." This paper is regarded as of great importance because it, in a certain sense, antedates Faraday's publications on the nature of atoms.

John P. Norton (1822-1852), teaching in Yale in 1847, wrote extensively on agricultural chemistry, so that he may be looked upon as a forerunner in that subject in this country. Under his teaching sat Wm. H. Brewer and S. W. Johnson, who, later, became pre-eminent in that field of chemistry. While among the first to give attention to agricultural chemistry, Norton did not become as well known in this field as Evan Pugh (1828-1864).

The following biographical sketch was written by W. I. Waring (1874):

Pugh, the first President of Pennsylvania State College, was, at 19 years of age, a blacksmith's apprentice. He bought out the residue of his time and supported himself by manual labour while he received a year's instruction at the seminary of Whitestown, N. Y. He had fallen heir to a small estate, including a private school, at Oxford, Chester Co. Pa. and moving hence, he conducted the school successfully for about two years.

In 1853 he decided to sell his estate and academy, which had become under his management a flourishing institution, in order to obtain means by which he might secure for himself a European course of scientific instruction. He went the same year to Europe and spent four years in the universities of Leipsic, Goettingen, and Heidelberg, and

in Paris. At Goettingen he honorably sustained the examinations for the degree of Doctor of Philosophy.

He contributed "Hamatinsalpetersäure identisch mit Pikraminsaure," "Miscellaneous Chemical Analyses," "On a New Method of Estimating Nitric Acid," "On the Sources of Nitrogen of Vegetation with Special Reference to the Question Whether Plants Assimilate Free or Uncombined Nitrogen." The last investigation was made in connection with Messrs. Lawes and Gilbert. It was while Pugh was in Paris that he wrote to Lawes announcing that he was ready to take up the study of this question, because it was one that was being widely discussed in France. Lawes, of course, said that he was willing to have the matter looked into, and invited Pugh to come to his laboratory, paying all expenses. The question which Pugh undertook to decide was one that had been raised more than half a century before by Priestley and Ingenhouse, on the one hand, who thought they had observed that plants absorbed the free nitrogen of the atmosphere, and Sennebier and Woodhouse, on the other hand, who negatived this opinion. In 1837, the matter was studied again by Boussingault, who had the sagacity to apprehend the importance of closely investigating the sources of the nitrogen periodically yielded by a given area of land, over and above that which was artificially supplied to it. After a series of experiments, extending over a period of 17 years, Boussingault concluded that plants did not assimilate free nitrogen. But it happened, in the meantime, that M. Georges Ville, of Paris, had, from a series of investigations made by him from 1849 to 1852, which seemed to show an enormous assimilation of nitrogen by the plants with which he ex-

perimented that could not be accounted for otherwise by him, announced that the free nitrogen of the atmosphere was assimilated by vegetation. Such strikingly different results at once excited great interest among chemists and vegetable physiologists, and a commission was appointed from the French Academy of Sciences to superintend the conducting, under M. Ville, of a new set of experiments at the Museum d'Histoire Naturelle in 1854-5. The report of this commission only tended to confirm the conclusions already drawn by M. Ville.

The researches, however, which were instituted by Pugh, and to which he devoted two years of nearly constant labor, were characterized with such comprehensiveness in their detail, skill, and ingenuity in the construction of apparatus and cautions against error, and, withal, such a rare degree of penetration to discover the many collateral questions involved, and acuteness in their solution, that the conclusions which they established have never since been questioned.

He returned home, in 1859, to assume a position which had been offered him—the presidency of the Agricultural College of Pennsylvania (Pennsylvania State College). Willingly renouncing the brilliant career which he was doubtless aware lay before him in case he should continue his researches, he recognized the duty he owed his country, and assumed the nobler and more enduring work. It was a controlling idea with him that the teacher lives a second generation in the mental development of the taught, and that to be a benefactor to his race the student must be the medium through which he should operate upon the great world around him.

When Pugh assumed the presidency of the new college the expediency of combining manual labor with thorough study in an institution of learning was an open question, all previous attempts of the kind, both in Europe and America, having resulted in signal failures. He had, however, perfect faith in a system which he believed was calculated, above all others, to develop mental and physical strength as well as practical knowledge. Referring once to the well known fact that it is not sufficient to have spent a certain number of years within the walls of a college or university in order to secure a respectable education, he said, "An English friend, himself a university graduate, once remarked to me that he could point to artisans in the workshops of England with better trained minds, as evinced by greater power of following up any connected train of thought, than could be found with many persons who had spent years at the time-honored universities of Oxford or Cambridge."

With the eyes of the friends of agricultural education in every civilized country resting upon the experiment, he had the courage to undertake to demonstrate its practicability. He had previously visited and carefully studied the chief agricultural academies and schools of Europe, and his idea of what an American agricultural college should be was as definite as it was comprehensive and just. He found the college a struggling institution, its buildings not half finished, and its exchequer awaiting the action of a hesitating legislature for funds to carry on the enterprise. With characteristic energy he organized a new plan of instruction, planned and superintended the erection of the college buildings, secured endowments, and, besides tak-

ing the general guidance of the institution, he gave instruction and superintended the practical investigations of the students in chemistry, scientific agriculture, mineralogy, and geology.

He had just succeeded in establishing a thoroughly scientific institution upon a broad and enduring basis, and in convincing a sceptical public of the ultimate success of such a noble enterprise, when death cut short his work at the age of 36.

Charles M. Wetherill (1825-1871), of the University of Pennsylvania, wrote "On the Neutral Sulphates of Ethyloxide, Their Decomposition Products with Water," "Analysis of the Subsulphate of Cinchona," "Experiments with Ammonium Amalgam," "On the Existence of the (so-called) Ammonium Amalgams," "Concretion from the Stomach of a Horse," "Molybdate of Lead," "Food of the Queen Bee," "Mexican Honey Ant," "A New Apparatus for the Determination of Carbonic Acid," "Examination of Fusel Oil from Indian Corn," etc. He studied under Liebig.

In this connection a paragraph may be devoted to the Rogers brothers. Years ago there lived in Philadelphia an Irishman by the name of Patrick Rogers, who, in due course, graduated from the Medical School of the University of Pennsylvania, and practiced medicine on Lombard Street for a few years. He had four sons, William B., Henry D., Robert E., and James B. At a very early age they went South with their father, who was called to teach Chemistry in one of the medical colleges of Baltimore,

and later in the College of William and Mary, where he succeeded Robert Hare. There is no record of any scientific work done by the father, but, when we consider his sons, American scientists may well honor the name of Rogers.

William B. was the founder and first president of the Massachusetts Institute of Technology. Henry D. was Chief of the Geological Survey of Pennsylvania, and afterward Professor of Geology in the University of Glasgow. James was more active in the promotion of technical or applied chemistry, especially in Philadelphia, where he followed the application of that science and succeeded to a remarkable degree. Upon the death of Robert Hare, he became his successor.

The fourth and youngest son, Robert E., was the chemist of the four. William B., the elder, was as a father to Robert, and it was under the direction of William and James that Robert E. received his training. His first chemical work was done when an assistant chemist on the Geological Survey, of which his brother, Henry D., was the Chief. In the *Journal of the Franklin Institute,* 1840, there appeared a paper on the determination of calcium as sulphate by Robert E. and Martin Boyé. The new method described by them is simply the precipitation of calcium by means of sulphuric acid and alcohol, while the second part of the paper relates to the separation of calcium from magnesium by the same method.

It may not be out of place to note that Martin Boyé was a Swede, who had graduated from one of the schools of technology in his country, and later (1844) took the degree of Doctor of Medicine from the University of Pennsylvania, and became a professor in the Boys' High School in Phila-

delphia. He wrote on the "Oxide of Cobalt, with Brown Hematite, of Chester Ridge, Pa.," "Analysis of the Bittern of a Saline on the Kiskiminetas River, Near Freeport, Pa.," "On the Perchlorate of the Oxide of Ethyl, or Perchloric Ether," two papers; "On Magnesium Limestones," two papers; "New Compounds of Deuto-chloride of Platinum, Nitric Oxide, and Chlorohydric Acid," two papers. There were others of minor importance.

But to return to Robert E. Rogers (1813-1884). In conjunction with his brother, William B., he worked out, at the University of Virginia, the method that is to-day used and known as the wet method for the determination of carbon. They employed it in the analysis of graphite and held that it was possible to use it in the analysis of the diamond. The method of oxidizing alcohol to aldehyde by the use of bichromate of potash and sulphuric acid was developed by Robert E. and William B. Rogers. One of the methods given in exhaustive treatises on Chemistry for the preparation of chlorine consists in heating bichromate of potash with concentrated hydrochloric acid. This method, also, was suggested and developed by Robert Rogers. While a student at the University of Pennsylvania, pursuing chemistry under Robert Hare, he presented a rather remarkable thesis. In the section of it relating to the action of animal and vegetable tissues, one reads:

My first object of attention has been to find whether or not there is a disparity in the rate of passage of gases through different structures. For this purpose four short tubes were chosen, equal in length and diameter. A portion of fresh cuticle recently separated from the *cutis vera* was tied across one end of the first. Over the second was fas-

tened a portion of peritoneum, over the third was a piece of mucous membrane, and over the fourth a very thin section of liver. These tubes being thus prepared and arranged over a mercurial trough, an equal measure of carbonic acid was passed up into each; a glass vessel was inverted over each of the tubes and filled with oxygen six times in volume of the carbonic acid in each tube. The opposite sides of the organic structures were thus in contact with different gaseous atmospheres. A rise of the mercury in each of the tubes was soon perceived, and the rate of movement was seen to be distinctly different in each. At the end of thirty minutes the experiment was suspended, being deemed satisfactory, and the mercury in the several tubes stood nearly as represented in the figure. We here perceive that from the third tube, where the mucous membrane was used, was the largest escape of the contained carbonic acid; a less proportion passed through the cuticle, a less share still through the peritoneum, and the least of all through the section of liver. . . . . These inquiries were repeated and extended to other membranes with similar results. . . . . As a deduction of the preceding determination, it seemed highly probable that by the use of certain tissues we might effect a separation of a particular gas from a mixture of two or more, so that by varying the tissue we might eliminate any gas at will, performing a species of proximate analysis. To test the truth of such an inference, two tubes were taken, and being bent into a rectangular elbow, one extremity of each was closed by a plate of metal perforated by a small round hole, corresponding to the caliber of the tube. A membrane being placed between the plates, they were then tightly clamped together. Thus arranged, I introduced into the leg of one of the tubes a measure of carbonic acid, a measure of oxygen, and a measure of hydrogen; and four measures of nitrogen were made to enter the other. The tissue employed in the first instance was mucous membrane. In

fifteen minutes the mercury stood elevated in the first tube and depressed in the second; and the experiment being stopped, the contents of the latter were examined. It was found that nearly the whole augmentation of volume in this tube was due to carbonic acid. Cellular tissue was now substituted in place of the mucous membrane, and after a longer time than in the previous case, when a similar change had arisen in the volume of gas in the two tubes, the contents of that tube which previously held the nitrogen were inspected, and were found to consist of some carbonic acid, a still greater proportion of oxygen, and all the nitrogen previously present. These experiments were extended to vegetable tissues . . . . . and always the general results were analogous to those above in showing a diversity of action according to the particular tissue and gases employed.

This story of the diffusion of gases through membranes naturally suggested an extension of these laws of action to liquids, and, by a unique series of experiments, involving the use of most diverse apparatus, he obtained the most convincing evidence of "the existence of an agency controlling the transmission of certain fluids in preference to others." "Perceiving that in many instances the liquids performed their movements in opposition to gravity, I was curious to ascertain if they would be able to overcome a greater mechanical resistance." The result of his experiments led him to say that "the force of transmission in this case (potassium sulphate) could not have been less than four atmospheres, and we are entitled to conclude that it would have been still greater had the membrane withstood the pressure. . . . . The laws of the transmission of fluids through organic structures are exhibited in re-

sults which are equivalent to a species of chemical decomposition." He next, by use of membranes, proceeded to separate gold, silver, and several other metals from their solutions, and was "led irresistibly to attribute an important office to the membrane itself."

The remainder of the investigation is marked by the same leaning toward experimentation, preferring rather that experimental results should guide in deducing his theoretical observations than uncertain speculation. This product of Robert Rogers' venture into the field of investigation shows great manipulative skill and the spirit of the true researcher. Whether the hand of the master, Robert Hare, was in any manner concerned in the progress of the study is not indicated. It seems to be an entirely independent contribution, and well deserved the recognition it received from the faculty to which it was presented.

The bibliography of Robert Rogers is as follows:

1. On the Alleged Insolubility of Copper in Hydrochloric Acid, Etc., Jour. Sci. and Arts, 2d Ser., Vol. VI (1848), p. 395.
2. On the Absorption of Carbonic Acid Gas by Sulphuric Acid, Etc. Chem. Gazette, Vol. VI, p. 477.
3. On the Volatility of Potassa and Soda and Their Carbonates. Proc. Am. Assoc. Adv. of Sci. (1848), p. 36.
4. On the Decomposition of Rocks by Meteoric Agents, Etc. Am. Jour. Sci. and Arts, 2d Ser., Vol. V, p. 401.
5. On the Absorption of Carbonic Acid by Liebig's Dilute Solution of Phosphate of Soda. Proc. Am. Assn. Adv. of Sci. (1848), p. 62.
6. On the Comparative Solubility of Carbonate of Lime, Etc., Proc. Am. Assn. Adv. of Sci. (1848), p. 95.
7. On the Absorption of Carbonic Acid by Acids and

Saline Substances. Proc. Am. Assoc. Adv. of Sci. (1850), p. 298.

8. Experiments on the Blood, Etc., Am. Jour. Med. Sci., Vol. XVII, p. 277.
9. On a New Process for Obtaining Pure Chlorine Gas. Am. Jour. Sci. and Arts, 2d Ser., Vol. I (1846), p. 428.
10. Report on the Consolidated Virginia and California Mines, in Annual Report of the Director of the Mint (1875), p. 81.
11. Report on the Equipment of the New Refinery in the Mint at San Francisco, Annual Report of the Director of the Mint (1875), p. 83.
12. On a New Process for Obtaining Formic Acid, Etc., Am. Jour. Sci. and Arts, 2d Ser., Vol. II (1846), p. 18.
13. On a New Process for Analyzing Graphite, Etc., Am. Jour. Sci. and Arts, 2d Ser., Vol. V (1848), p. 352.
14. Oxidation of the Diamond in the Liquid Way, Am. Jour. Sci. and Arts, 2d Ser., Vol. VI (1848), p. 110.
15. On the Decomposition and Partial Solution of Minerals, Etc., by Pure Water. Am. Jour. Sci. and Arts, 2d Ser., V (1848), p. 401.
16. On the Use of Hydrogen Gas to Displace Sulphuretted Hydrogen in the Analysis of Mineral Waters. Am. Jour. Sci. and Arts, 2d Ser., Vol. XVIII (1854), p. 213.
17. On the Analysis of Limestones, Etc., Jour. Frank. Inst., Vol. XXV (1840), p. 158.
18. Report upon the Wastage of Silver Bullion, Etc. Gov. Printing Office, 1872.
19. Report on Dynamo-electric Machines. Jour. Franklin Inst., 3d Ser., Vol. XXV (1878), p. 289.
20. Report of the Committee on the Precautions to Be Taken to Obviate the Dangers of Electric Lighting. Jour. Franklin Inst., 3d Ser., Vol. XXXII (1881), p. 401.

Theodore G. Wormley (1826-1897) succeeded Robert E. Rogers as Professor of Chemistry in the Medical School of the University of Pennsylvania. His first professional appointment came in the year 1852, when he was elected to the Chair of Chemistry and Natural Science in the Capitol University, Columbus, Ohio. This position he held until July, 1865. During the same period he also held the professorship of Chemistry and Toxicology in Starling Medical College, to which he had been elected in 1854, and from which he resigned after twenty-three years of most satisfactory work. The hours not devoted to teaching or research were occupied in the discharge of appointments, for which his qualifications admirably fitted him, such as State Gas Commissioner of Ohio (1867-1875), and Chemist of the Geological Survey of Ohio (1869-1874). In both these positions he rendered most distinguished service, as is amply evidenced by the various State Reports in which his records are published.

His acknowledged reputation as a teacher and toxicologist led to his election, on June 5, 1877, to the Chair of Chemistry and Toxicology in the Medical Department of the University of Pennsylvania, thereby becoming the direct and worthy successor of such eminent men as Benjamin Rush, James Hutchinson, James Woodhouse, J. Redman Coxe, Robert Hare, and Robert E. Rogers. And here he continued until the morning of January 3, 1897, when the final summons came, and the earnest, ever-active master laid aside the working tools of life to penetrate the veil which separates us and the present from the great hereafter.

The results of his researches appeared under the following titles:

1. On Some of the Chemical Reactions of Strychnia. Chem. News, 1860.
2. Notes on Some of the Chemical Reactions of Atropine. Chem. News, 1860.
3. Notes on Some of the Chemical Reactions of Brucine. Chem. News, 1860.
4. Chemical Reactions of Corrosive Sublimate. Chem. News, 1860.
5. Chemical Reactions of Morphia. Chem. News, 1860.
6. Chemical Reactions of Narcotine and Meconic Acid. Chem. News, 1860.
7. Nobert's Test Plate and the Striæ of Diatoms. Chem. News, 1861.
8. Quantitative Estimation of Urea. Chem. News, 1882.
9. Recovery of Absorbed Morphia from Blood. Chem. News, 1891.
10. A Contribution to Our Knowledge of the Chemical Composition of Gelsemium. Am. J. Pharm., 1870.
11. Alkaloids of Veratrum Viride and Alum. Am. Jour. Pharm., 1876.
12. Preparation and Toxic Effects of Gelsemia. Am. J. Pharm., 1877.
13. Reinsch's Test Fallacies. Am. J. Pharm., 1880.
14. Constitution of Gelsemium. Am. J. Pharm., 1882.
15. Some of the Chemical Properties of Mydriatic Alkaloids. Am. J. Pharm., 1894.
16. Tests for Quinine. Am. J. Pharm., 1894.
17. Recovery of Absorbed Morphine from the Urine, the Blood, and the Tissues. Univ. Med. Mag., 1889-90.
18. Concordant and Micrometric Measurements. Univ. Med. Mag., 1890-91.
19. Chemical Analysis of Coals, Iron Ores, Etc. Ohio Geol. Survey, 1870.

Editor of the Ohio Medical and Surgical Journal, 1862-1864.

These, in a measure, indicate the direction of his activity, but the lasting monument which he raised to science and his own glory is his "Micro-Chemistry of Poisons." In it are embodied the records of thousands of the most painstaking observations. The patience displayed in the preparation of this volume of world-wide reputation—the recognized authority in all lands—is marvellous. It is interesting also to note that in this, his greatest effort, he was assisted by his devoted wife, who learned the art of steel engraving solely for the purpose of delineating upon steel nearly one hundred exquisite illustrations of crystals, drawn directly from the object as observed under the microscope.

Wormley was the recipient of many honors, and received elections to many scientific societies. He was one of the vice-presidents of the Centennial of Chemistry, held at Northumberland, Pa., in 1874; a member and vice-president of the American Chemical Society; a member of the American Philosophical Society; a member of the American Metrological Society; a corresponding member of the New York Medico-Legal Society; a Fellow of the College of Physicians, Philadelphia; a Fellow of the American Association for the Advancement of Science; and a Fellow of the Chemical Society of London.

CHAPTER XI

A POTENT factor in American chemistry was James C. Booth (1810-1888), of Philadelphia. Deciding to follow chemistry as a profession, he went to Germany in December, 1832, and entered Frederick Wöhler's private laboratory in Cassel, there being at that time no university laboratories arranged for the regular reception of students; and it is believed that he was the first American student to study analytical chemistry in Germany. After a year with Wöhler, he went to Berlin, and spent an equal amount of time with Gustav Magnus. The remainder of his three years abroad was devoted to the practical study of chemistry applied to the arts in the manufacturing centers of the Continent and England.

With an education probably unequalled at that time by any chemist in America, he returned to the United States, and, in 1836, established in Philadelphia a laboratory for instruction in chemical analysis and applied chemistry. This institution soon acquired considerable distinction, being the first of its kind in this country, and during the course of a few years nearly fifty students availed themselves of his instruction, most of whom have since acquired distinction. The list included John F. Frazer, professor of chemistry at the University of Pennsylvania in 1844-72; Thomas H. Garrett, who became his partner in the analytical business and survived him; Robert E. Rogers, professor

of chemistry at the University of Pennsylvania in 1852-77, etc.

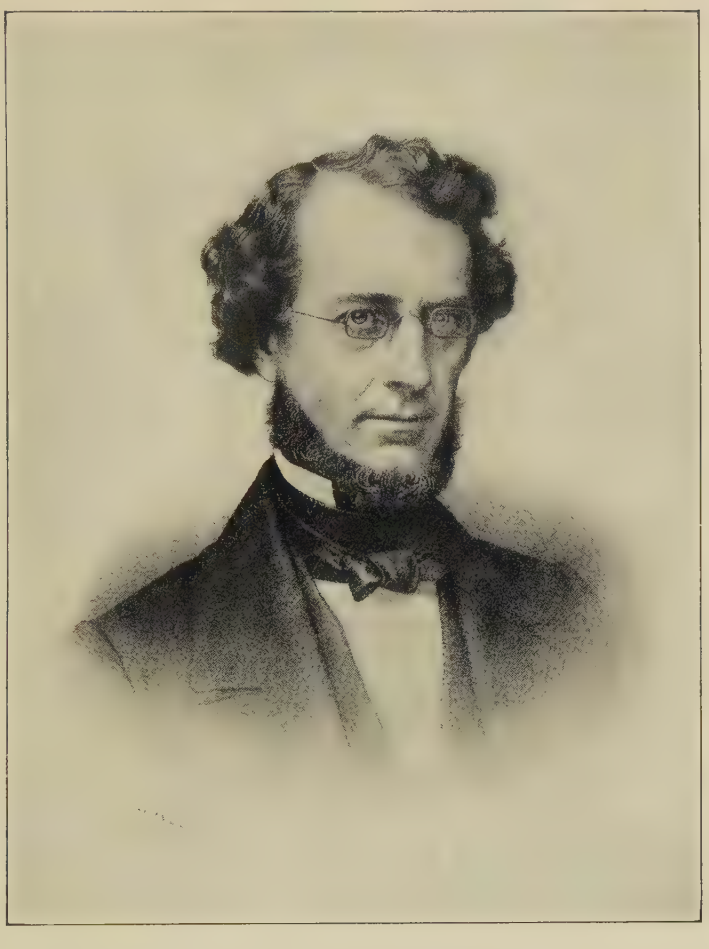
At first he was assisted by Dr. Martin H. Boyé, who remained with him until 1845, and in 1848, Thomas H. Garrett became his associate. The latter continued to manage the analytical part of the business until 1881, when Andrew A. Blair joined the firm. The business was then conducted under the title of Booth, Garrett, and Blair; it was known for the high grade of analytical work performed, especially in the examination and determination of iron ores.

In 1849 Booth received the appointment of melter and refiner at the U. S. Mint in Philadelphia, which office he held until his death. . . . . In his official capacity he was frequently consulted by the government on questions pertaining to chemistry, and his studies on the nickel ores of Pennsylvania led, in 1856, to the adoption of nickel as one of the components of the alloys used in the coinage of the cent issued in that year.

During 1837-39 he had charge of the geological survey of the State of Delaware. He was a member of the American Philosophical Society and of the Philadelphia Academy of Sciences. He also served as President of the American Chemical Society from 1884 to 1885.

An active participant in the upbuilding of chemistry in America was T. Sterry Hunt (1826-1892), of whom Dr. Marcus Benjamin writes so charmingly that his words will be given:

The name of no American chemist occurs more frequently, or in a more important relation to the progress



James C. Booth

and development of our science, than that of Dr. Hunt. His contributions to our science have been equally valuable in theoretical chemistry, in chemical philosophy, and in geological and mineralogical chemistry.

He was born in Norwich, Conn. . . . . His early education was acquired in Norwich, but he was attracted to New Haven by the fame of the scientific development there in progress under the elder Silliman. He accepted, in 1847, the post as chemist and mineralogist to the Geological Survey of Canada, which place he held for twenty-five years. He also occupied the chair of chemistry in Laval University, Quebec, from 1856 to 1862, delivering the lectures in French; and, thereafter, until 1868, he filled a similar appointment at McGill University, in Montreal. In 1872 he returned to the United States and accepted the chair of geology in the Massachusetts Institute of Technology made vacant by the resignation of William B. Rogers. This appointment he held until 1878, since when he devoted his attention chiefly to expert work and literary pursuits.

No author has covered a wider range in chemistry than he. Not less than one hundred and thirty entries are found under his name in the second and third series of the *American Journal of Science;* and, adding those published in Canada, England, and France, and some memoirs in the proceedings of various American Societies, the total roll of his papers amounts to about one hundred and sixty titles.

The views of Laurent and Gerhardt found their first advocacy in this country at the hands of Hunt in his able review of the *Precis* of the latter in 1847, and his own papers in the years next following have contributed in no small degree to extend the bounds of theoretical chemistry and its philosophy. Particular mention may be made of his paper 1. On the Anomalies in the Atomic Volume of Sulphur and Nitrogen, in 1848. This paper contains also remarks on chemical classification and a notice of Laurent's Theory of Binary Molecules. In his paper 2. On Some

Principles to Be Considered in Chemical Classification, read at the Philadelphia meeting of the American Association in 1848, Hunt freely criticizes the systems of Liebig and of the French School, the rather to show their merits than their defects, and to exhibit their real harmony with each other and with nature. In this paper he advances his own views, showing what is now recognized as the true constitution of gaseous nitrogen—NN—and that the various saline forms are reducible to two, the types of which are seen in water, H_2O, and the protoxyds, M_2O, and in the hydrogen, H_2, or the metals M_2, the first including all the oxygenized acids, and the second the hydracids. 3. On the Chemical Constitution of Gelatine and Its Transformations.

4. Remarks on the Constitution of Leucine and the Ureas.

5. On the Compound Ammonias and the Bodies of the Cacodyle Series.

6. On the Action of Sulphuretted Hydrogen Upon Nitric Acetene.

7. On the Decomposition of Aniline by Nitrous Acid.

8. On the Theoretical Relations of Water and Hydrogen. In this paper, published in March, 1854, he reviews the opinions of the European chemists on the water-type, and reclaims for himself the priority of authorship in this important conception which the English edition of Gmelin's Handbook ascribes to Williamson.

9. On the Theory of Types in Chemistry, is the title of a memoir, dated January 5, 1861, in which there is ably reviewed the history of the subject, and shown that, in the series of papers whose titles are above quoted, 1 to 9, were first developed the views of the water-type and of multiple or condensed types which were subsequently adopted by Williamson, Gerhardt, and Ad. Wurtz. Wolcott Gibbs, in an essay presented by him at the Baltimore meeting of the American Association for the Advancement

of Science, May, 1858, remarks that in a previous paper of his (his "Report of the Progress of Organic Chemistry") he had attributed the theory of water-types to Williamson and Gerhardt, and adds, "in this I find I have not done justice to T. Sterry Hunt, to whom is exclusively due the credit of having first applied the theory to the so-called oxygen-acids and to the anhydrids, and in whose earlier papers may be found the germs of most of the ideas on classification usually attributed to Gerhardt and his school."

10. Theory of Nitrification and Nature of Gaseous Nitrogen further developed with experiments on the oxidation of nitrogen by permanganic acid, and the origin of nitrous acid, forming a key to the true origin of nitrites and nitrates in nature. This view was adopted without change or addition by Schönbein in 1862, and without acknowledgment.

Some of Hunt's more important contributions to Chemical Philosophy are "Consideration of the Theory of Chemical Changes," and on "Equivalent Volumes," "Thought on Solution and the Chemical Process." In this paper the ground is taken that all solution is chemical union. "On the Objects and Methods of Mineralogy." "On the Constitution and Equivalent Volume of Some Mineral Species," "Illustration of Chemical Homology."

His researches on the equivalent volumes of liquids and solids were a remarkable anticipation of those of the great French chemist, Dumas, while, in his "Introduction to Organic Chemistry," published in 1852 with Silliman's "First Principles of Chemistry," he was the first to define that branch as "the chemistry of carbon and its compounds." His studies of the polymerism of mineral species, as set forth in his paper on "Objects and Methods of Mineralogy," opened a new field for mineralogy, but these philosophical studies were only incidental to his labors in chemical mineralogy and chemical geology.

His researches into the chemical and mineral composition of rocks were probably more extended than those of any contemporary scientist. From his long series of investigations of the lime and magnesia salts he was enabled to explain for the first time the relations of gypsums and dolomites, and to explain the origin of the latter by direct deposition. The first systematic attempt to subdivide and classify geologically the stratiform crystalline rocks was made by him. The names Laurentian and Huronian, applied to the earliest known rocks on this continent, were given by him to two subdivisions of the Azoic period. Likewise the distinction and designations of Norian, Montalban, Taconian and Kekeenian were originated by him and have gained an acceptance in the literature of geology. In connection with these studies he attempted the discussion of the great questions of the origin and the succession of these rocks.

He sought to harmonize the facts of dynamical geology with the theory of a solid globe, and, after reviewing and controverting various hypotheses, including the igneous or plutonic, the metamorphic, and the metasomatic, all of which he rejected as irreconcilable with observed facts and as isolating chemical theory, thus showing the essential correctness of the still imperfect Wernerian aqueous view, he advanced the so-called crenitic hypothesis, in which he argued that the source of the various groups of crystalline rocks was the original superficial portion of the globe, once in a state of igneous fusion, but previously solidified from the center. This portion, rendered porous by cooling, was permeated by circulating water, which dissolved and brought to the surface during successive ages, after the manner of modern mineral springs, the elements of the various systems of crystalline rocks. These views were originally advanced in his essay on the "Chemistry of the Earth," which was published in the "Report of the Smithsonian Institution" for 1869.

His conclusions on many points of geology are embodied in his retiring address as president of the American Association for the Advancement of Science, at Indianapolis, in 1871, and in a matured form in his "Mineral Physiology and Physiography," originally published in Boston in 1886, in which may be found his theories of the origin, development, and decay of crystalline rocks set forth in detail.

Hunt was the first to make known the deposits of phosphate of lime in Canada, and to call attention to their commercial value for fertilizing purposes. The chemical and geological relations of petroleum were studied by him, and the salt deposits of Ontario were investigated by him. His researches in the chemistry of mineral waters were exhaustive, and were said to have been "more extended than those of any other living chemist." Reports and papers on these subjects by him appeared in the various volumes issued by the Geological Survey of Canada.

In 1859 he invented and patented the permanent green ink, which has since been so extensively used, and gave the name of "greenback" currency to the bills which were printed with it. Later he was associated with James Douglass, Jr., in the invention of a wet process for the extraction of copper from low grades of ores, consisting essentially of roasting the ore, bringing it into solution, and then precipitating the copper in its metallic form by the introduction of iron.

Besides the reports of the Geological Survey of Canada, he published in book form, "Chemical and Geological Essays" (Boston, 1874, 4th ed. New York, 1891); "Azoic Rocks," being "Report E of the Second Geological Survey of Pennsylvania" (Philadelphia, 1878); "A New Basis for Chemistry" (Boston, 1887, 2d ed. New York, 1890). This also appeared as "Nouveau Systeme Chimique" (Paris, 1889), and a Russian translation, being the initial volume of a series of foreign scientific classics. His last work, entitled "Systematic Mineralogy According to a

Natural System," was published in New York during 1891.

He was president of the American Association for the Advancement of Science in 1870 and of the American Institute of Mining Engineers in 1877. The American Chemical Society called him to its presidency in 1880, and again in 1888. He was one of the founders, and the first president by election, of the Royal Society of Canada, in 1884. In 1876 he organized, in concert with American and European geologists, the International Geological Congress, was its first secretary, and vice-president at its meetings held in Paris, in 1878; in Bologna, Italy, in 1881; and in London, in 1888.

In 1859 he was elected a Fellow of the Royal Society of London, and in 1873 he was chosen to the National Academy of Sciences. He was a member of the American Philosophical Society, the American Academy of Arts and Sciences, and abroad he was a member of the geological societies of France, Belgium, Austria, Ireland, and of a number of other scientific bodies.

Shortly after he had retired from active work this was written of him: "Although an indefatigable experimenter and an extensive observer, Hunt was also eminently an original and philosophical thinker, and took an influential part in the establishment of the most matured and scientific theories. He was early in the field of chemical speculation, and aided essentially in that revolution of views which has ended in the establishment of a new chemistry."

Eminent and enthusiastic as a worker in the field of chemistry was John Lawrence Smith (1818-1883), of Charleston, S. C. His taste for physical phenomena and for scientific pursuits led him to the University of Virginia, where, before he was seventeen years of age, he pursued chemistry, natural philosophy, and civil engineering.

T. Sterry Hunt

The account of his life, given in the following paragraphs, has been condensed from a sketch published in the *Biographical Memoirs of the National Academy of Sciences* and from one privately printed in Louisville, Ky., by intimate friends:

On graduating from the Medical College of South Carolina, in 1840, he submitted a thesis on the "Compound Nature of Nitrogen."

Immediately after taking his medical degree Smith went to Paris. With Dumas he studied pure and applied chemistry, and with Orfila toxicology; with Poulliet, Desprez, and Ed. Becquerel, physics; and with Dufrenoy and Elie de Beaumont, mineralogy and geology.

During one of his summer excursions he found himself at the door of Liebig's laboratory in Giessen. This accidental circumstance turned the whole course of his life into the channel from which it was never afterward diverted. He became a zealous and enthusiastic student of chemistry under the inspiration of Liebig. He appears to have divided his time between Giessen in the summer and Paris in the winter.

While yet a student in Charleston, he wrote on "Chromate of Potassa—a Reagent for Distinguishing between the Salts of Baryta and Strontia." In this paper he first made known to chemists this very delicate and now very familiar test for barium, and determined its quantitative value. In the same year he published "A New Method of Making Permanent Artificial Magnets by Galvanism."

In 1842 he transmitted from Paris a research "On the Composition and Products of Distillation of Spermaceti, Etc."

At the time when this research was made probably no American chemist had done any work in organic chemistry of so elaborate a character as is shown in this investigation.

In a subsequent paper on the action of potash on cholesterine he showed that this body was nearly related to spermaceti.

Shortly after Dr. Smith reached Paris he found the whole of France agitated by one of the most interesting criminal processes upon record—that of Madame La Farge. This case involved the question of the normal existence of arsenic in the human body, and of its presence in hydrated peroxide of iron, used as an antidote. Dr. Smith reviewed these and other questions in a paper entitled "On the Means of Detecting Arsenic In the Animal Body, and of Counteracting Its Effects." The date of the Paris paper is December 6, 1840. He was then a student of Orfila, but did not hesitate to expose the errors of his distinguished professor in this celebrated case—errors afterward acknowledged by Orfila himself.

He followed up this subject by a second paper of date June 28, 1841, entitled "Continuation of Remarks Made Upon Arsenic, Considered In a Medico-Legal Point of View," in which he described the method of Danger and Flandin for destroying animal matter in toxical examinations, and also the then new form of Marsh's apparatus; and he still further reviewed the ground for rejecting the opinion that arsenic is ever a normal constituent of the animal body, supporting his statements by evidence from his own original work.

In November, 1841, Smith sent to this country from

Giessen a translation of Will and Warrentrapp's method of determining nitrogen in organic compounds, accompanying it with notes of his own.

Before 1846 he had done some important work in chemical analysis and in the improvement of analytical methods; thus determining the action of alkaline salts on sulphate of lead; the composition of fossil bones from near Charleston, and origin of the fluorine found in them; the action of solutions of the neutral phosphates upon carbonate of lime, and the composition of marl from Ashly River, S. C.

These papers undoubtedly led to his selection by Secretary (later President) Buchanan as a suitable person to meet the call from the Sultan of Turkey for scientific aid in introducing into that kingdom American methods in the culture of cotton—a subject with which he was also familiar.

Finding on his arrival in Turkey that an associate proposed to inaugurate the cultivation of cotton on a plan doomed to failure, he was about returning to America when he received from the Turkish government a commission to explore her mineral resources. He entered at once upon this work, and, in the four years of his residence in the Sultan's dominions, opened up natural resources which have ever since added an important item to the revenues of the Porte.

His memoir on "emery" was equally important. From the study of the mineralogical associations in which he found the emery of Asia Minor, he felt convinced that the search for like associations elsewhere would be rewarded by the discovery of emery or corundum.

After his return to America he had the opportunity of

seeing the accuracy of his views demonstrated at the mine at Chester, in Hampden County, Mass.

Smith's memoir on the Turkish emery was presented to the French Academy in 1850, and published.

Many other researches, scientific and economic, on coal, chromite, magnesite, etc., were prosecuted by Smith while in the service of the Ottoman government.

In the early spring of 1850 he returned to Paris and remained there until the following October, occupied with scientific work relating to emery and its associate minerals, and the presentation of his two memoirs on this subject to the Institute.

He also found time to project his inverted microscope, which he matured after his return home.

January, 1881, he wrote from Charleston, "I often regret that I am not more permanently established, for my concentration on scientific labor can never be made advantageous until I have a well mounted laboratory of my own—In fact I have been literally a sort of peripatetic philosopher, carrying my own hammer and anvil and doing a little wherever I could get a place to work in. It would no doubt surprise you to see in my baggage a box of platinum, from a pint capacity down; bottles of pure carbonate soda, bisulphate of soda, fluorspar, potash, carbonate of lime, etc.; in fact, my essentials that I am only satisfied of as to purity when they come from my own stock."

In a letter, of date January 7, 1853, he said, "My method of analyzing the alkaline silicates is now complete, and will appear in the next number of the *American Journal of Science*. This method of decomposing silicates for the al-

kalies is quite as easy as a carbonate of soda fusion, which latter, however, is an insignificant decomposing agent along side of it. Zircon and kyanite yield to it at a light red heat in an open furnace. Carbonate of lime is the agent. You will learn how to use it for this purpose by referring to the forthcoming paper."

This paper appeared in March, 1853,* and it was a very valuable contribution to analytical methods; Smith's process for decomposing the alkaline silicates, by the use of calcium carbonate and chloride, is now the generally accepted method.

The researches on American minerals, carried on jointly by Smith and Brush, were made in 1853, and have long since passed into the records of science. They settled many doubtful points, and relegated into obscurity many worthless species, while clearly establishing others.

In 1854 Smith accepted the chair of medical chemistry and toxicology in the University of Louisville. This chair he retained until the spring of 1886. Possessed of an ample fortune, and frequently called, in the way of his profession, to visit Europe, he found the restraints of a professorship, in an institution no longer prosperous, distasteful, and naturally preferred to devote himself to the more congenial researches which he had commenced in the department of aerolites, to the collection and study of which he gave great attention during the remainder of his life. His first memoir on this subject was his description of five new meteoric irons in 1854, forming part of his memoir on meteorites, read before the American Associa-

* *Am. Jour. Sci.* (2) XV, 234-243. Completed in Part II, July 1, 1853. (2) XVI, 53-61.

tion for the Advancement of Science in April, 1854, but not published until the following year.*

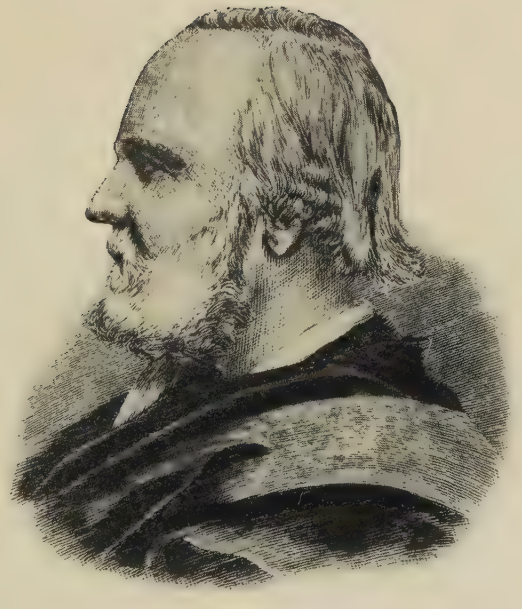
In this paper Smith appears for the first time as the author of a general theoretical discussion of cosmical or astronomical considerations as to the origin of meteorites. His views are expressed with force and clearness. He antagonizes the notion that meteorites, as we know them from the fragments which reach the earth, are large, solid cosmical bodies, passing through the earth's atmosphere with planetary velocity, and dropping small portions of their mass in their flight. He advocates strongly the lunar origin of meteorites as the most probable theory yet advanced. This view he sustains with the courage of his convictions, and illustrates by citing many interesting facts, which, in his view, go far to establish the lunar theory.

If we turn to the list of his papers in the Royal Society catalogue, we find, out of seventy-eight titles, down to 1872, there are twenty-two upon meteoric subjects, all subsequent to 1854, and of these seventeen papers were printed between 1864 and 1873, the date of the publication of his volume of papers already cited; in this volume of four hundred pages one hundred are devoted to meteorites. Succeeding these are seventeen additional meteoric papers and many on other subjects, chiefly mineralogical.

The last paper printed by Smith, 1882, was "On the Peculiar Concretions in Meteoric Iron." At the close of this paper he says he will continue the research, "if," he adds, "my health permits." It was his last work.

Smith's collection of meteorites was commenced by his purchase of the valuable collection of G. Troost, of Nash-

* *Am. Jour. Sci.* (2) XIX, 153-332.



J. Lawrence Smith

ville University, who was fortunate in securing a number of large iron meteorites from Tennessee, but Smith added constantly to this collection from all parts of the world, and especially of irons from Mexico, as well as from the United States, and of stones from such falls as those of New Concord, Ohio, in May, 1860, and from the great fall of Iowa in February, 1875; Nash County, North Carolina, 1872; Warren County, Missouri, January 7, 1877; and others. He sold a number of his larger iron masses in 1862 to Prof. C. U. Shepard in London, and of the Mexican irons to the museum of the Garden of Plants in Paris (1879).

Fortunately for science this fine meteoric collection has passed, entire, by purchase, into the possession of Harvard College.

Smith's collection is, in a sense, a monumental one, memorial of the life work of a devoted student in this very interesting department of cosmical chemistry and mineralogy.

Among others, Smith was a member of the National Academy of Sciences and, in 1879, corresponding member of the Academy of Sciences of the Institute of France.

The list of his published contributions to chemical science numbers 145. The last refers to "Methods of Analyzing samarskite and the other columbates containing earthy oxides by the agency of fluorhydic acid; and of dissolving columbite and tantalite by the same acid. On the separation of thoria from the other oxides. Quantitative estimation of didymium oxide in its mixtures with other earthy oxides" (1881).

The writer met J. Lawrence Smith in the laboratory of Genth (1879). It was his privilege to observe these two

eminent and brilliant chemists at work. They had differed on a point in the analysis of a complex silicate and, rather than cover valuable pages of some journal with their differences, occupying space that could be better used, they determined to settle the point at issue by experimental demonstrations made in each other's company. In this particular case Genth was in the right.

It was a further privilege of the writer to see Gibbs, Genth and Smith working amicably and cheerfully in the same room over problems which greatly interested them. Some of the difficult points in the analytical study of the cobaltamines were settled by Gibbs and Genth with the writer a very silent but much interested observer and auditor of the work and remarks of these pioneers.

Smith appeared in the laboratory of the University where the writer was an assistant. He was then short and stout of figure. His hair was heavy, thick, quite gray, parted on one side, rather long and brushed back. He wore spectacles which rested on the end of his nose. On learning that the writer was endeavoring to procure a compound ether by heating the silver salt of an organic acid with ethyl iodide, Smith, having seated himself on a high stool, remarked "that is a new method to me." On several occasions the writer enjoyed hearing from Smith the story of his study of the rare earths and, in particular, his experiences in unravelling the composition of samarskite, in which he was especially interested.

The reader will pardon this personal digression on the part of the writer, but to him it has always seemed as if there could not be too much credit given Genth, Gibbs and J. Lawrence Smith for the admirable contributions they

made to the development of chemical science in the United States.

And, now, to the life history of Frederick A. Genth (1820-1893), born in Hesse in 1820.

His university career began in Heidelberg, where he heard Gmelin in chemistry. He studied for a year and a half at Giessen under Fresenius, Kopp and Liebig. In 1844 he continued his chemical studies under Bunsen at the University of Marburg. It was from this University that he received the degree of Doctor of Philosophy.

It was about 1848 that he came to America, and, after occupying several positions and conducting a laboratory for commercial analysis and the instruction of special students in chemistry, he became Professor of Chemistry in the University of Pennsylvania, in 1872.

His earliest contributions were upon geological subjects. Later he devoted much time to mineralogical problems. The chemical research by which he is best known relates to the ammonia cobalt bases (the cobaltamines) developed jointly with Wolcott Gibbs. His original memoir was published in Philadelphia in 1851 and contained the first distinct recognition of the existence of perfectly well defined and crystallized salts of the ammonia cobalt bases. The joint monograph of Genth and Gibbs appeared in 1856. This elaborate and extended research has always stood among the finest chemical investigations ever made in this country. Several years were required to complete it, the analytical portion of the work being as difficult as it was protracted.

In 1858, Genth and Gibbs published a notice of a new base containing osmium and ammonium. The cobaltamines

of Genth and Gibbs were never fully understood as to their constitution until in recent years, when they have been interpreted so admirably by the painstaking efforts of Werner.

The chief chemical investigations of Genth, which are most highly valued, are those in connection with mineralogy. His contributions to this subject are fifty-four in number. The contents describe 215 mineral species. Genth was the discoverer of twenty-four new mineral species, all of which were so thoroughly investigated, both by chemical and physical methods, that they found at once a position in the science which they have ever since maintained.

"Corundum, Its Alterations and Associated Minerals" is the title of one of his important and extended studies. It appeared in 1873, in the "Proceedings of the American Philosophical Society." It occupied fifty-six pages.

Genth was chemist for the second Geological Survey of Pennsylvania and chemist to the Board of Agriculture of Pennsylvania, and did much by his investigations, especially by his analysis of fertilizers, to develop the agricultural industry of the State of Pennsylvania and to maintain a high standard of excellence in all farm products.

As a chemist, he was almost without a peer, especially in the field of analysis, being familiar not only with the reactions and methods of determination and separation of the ordinary elemental and compound ions, but, what is more remarkable, with those of the rarer and less frequently occurring ones as well; but, more than this, his scientific work was characterized by a conscientiousness and fidelity to fact which was exceptional. His knowledge of minerals was complete.

He was a member of the American Philosophical Society, President of the American Chemical Society, a member of the National Academy of Sciences, a Fellow of the American Academy of Arts and Science, and an Honorary Fellow of the American Association for the Advancement of Science.

He died in 1893. His contributions to chemical and mineralogical subjects number 102.

CHAPTER XII

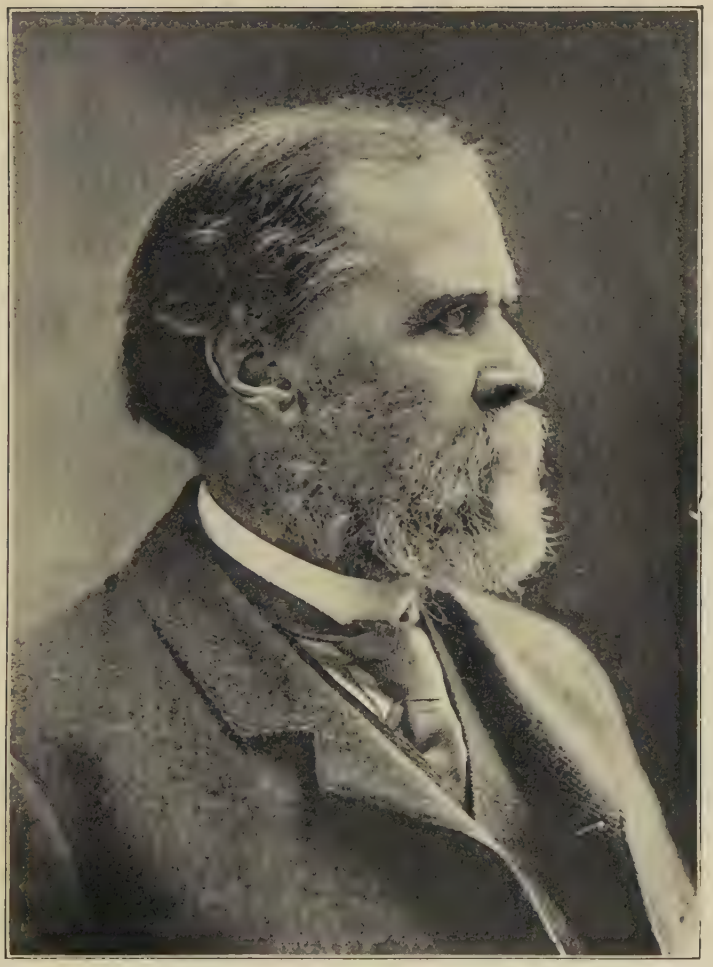
OF the eminent chemists who, from time to time, congregated in the laboratory of Genth, none attracted and absorbed the youthful attention of the writer to so great an extent as Wolcott Gibbs (1822-1908).

He was, indeed, a master in the science and an inspiration to all who were fortunate enough to meet him. Frank Wigglesworth Clarke has written so beautifully of this exceptional investigator that it seems most appropriate that his sketch should be given as much publicity as possible, and it is accordingly appended in almost its exact language:

Wolcott Gibbs for years held the most commanding position among the chemists of the United States.

He was born in New York City (1822). He received his Bachelor's degree from Columbia University in 1851, after which he served as assistant to Robert Hare in Philadelphia. In 1845 he received his Doctorate in Medicine. He, however, never practiced medicine. Subsequently, he studied chemistry with Rammelsberg in Berlin and spent one year with Heinrich Rose, which was followed by a semester with Liebig at Giessen. In Paris, he attended the lectures of Laurent, Dumas, and Regnault. He returned home in 1848.

In 1849 he became Professor of Chemistry in the College of the City of New York, which chair he held for fourteen years.



Wolcott Gibbs

In 1857 his first notable research appeared, namely, the joint "Memoir of Genth and Gibbs on the Ammonia Cobalt Bases." In 1861 the first of his papers on the "Platinum Metals" appeared.

In 1863 he became Rumford Professor in Harvard University. He was in charge of the laboratory of the Lawrence Scientific School for eight years.

It was Gibbs' peculiar merit, that he, more than any other man, introduced into the United States the German conception of research as a means of chemical instruction.

His first paper was a "Description of a New Form of Magneto-Electric Machine, and an Account of a Carbon Battery of Considerable Energy." This he published while yet a junior in college. In 1844 he attempted to discuss the theory of compound salt radicles. In 1850 he pointed out the interesting fact that compounds which change color when heated do so in the direction of the red end of the spectrum. In 1852 he published his first memoir on analytical methods; in 1853 he prepared an arsenical derivative of valeric acid. Mineral chemistry, organic chemistry, analytical chemistry, chemical theory and physics in turn attracted his attention during this formative period of his career. It was in the great research upon the ammonia cobalt bases, to which reference has already been made, that Gibbs finally found himself and forced the world to recognize his ability. In the celebrated memoir of Genth and Gibbs thirty-five salts were described of the four bases roseocobalt, purpureocobalt, luteocobalt, and xanthocobalt. The roseocobalt and purpureocobalt compounds were for the first time clearly discriminated. There was also an elaborate theoretical discussion upon the constitution of

the bases, but that was premature. Genth and Gibbs laid the foundations on which later investigators have built an imposing structure.

In 1867 Gibbs published a paper upon atomicities or valences in which he developed the idea then vaguely held by others of residual affinities. He argued in favor of the quadrivalency of oxygen, and showed that on that supposition a molecule of water must be bivalent, and any chain of water molecules would be bivalent also. He then considered ammonia in the same way, with the two bonds of quinquivalent nitrogen unsatisfied. Ammonia, therefore, was weakly bivalent, and so, too, would be a chain of ammonia molecules. This conception he applied to the interpretation of the ammonia cobalt bases, and so, too, did Blomstrand two years later. If we consider theories of this kind not as finalities, but as attempts to express known relations in symbolic forms, we must admit that Gibbs' conception was useful, and served well for the time being. In the later papers by Gibbs, published in 1875, he made good use of his hypotheses, and described many more ammonia-cobalt compounds. Among them were the salts of an entirely new base, croceocobalt, in which two nitro-groups were present. In all, five distinct series were studied, their chlorides being represented, in modern notation, by the subjoined formulas:

Luteocobalt chloride ..... $Co(NH_3)_6Cl_3$
Roseocobalt chloride ..... $Co(NH_3)_5H_2O.Cl_3$
Purpureocobalt ..... $Co(NH_3)_5Cl.Cl_2$
Xanthocobalt chloride ..... $Co(NH_3)_5NO_2.Cl_2$
Croceocobalt chloride ..... $Co(NH_3)_4(NO_2)_2Cl.$

Gibbs' formulas were somewhat different from these, being doubled, and with the water of roseocobalt regarded not as constitutional, but as crystalline. The simpler, halved expressions were established by cryoscopic methods which did not exist when Gibbs conducted his investigations.

The researches upon the platinum metals, published by Gibbs in the years 1861 to 1864, relate mainly to analytical methods. Processes for the solution of iridosmine were carefully studied, and various new separations of the several metals from one another were devised. Incidentally, a number of new compounds were prepared, which, with a few exceptions, Gibbs never fully described. In 1871, however, he published a brief note on the remarkable complex nitrites formed by iridium,* and in 1881 he described a new base, osmyl-ditetramin, $OsO_2.4NH_3$, together with several of its salts. These researches were never pushed very far, and were discontinued for lack of proper facilities. They were, nevertheless, distinct additions to our knowledge of the platinum group.

With his students he covered a wide range, partly in developing and perfecting old analytical methods, partly in devising new ones. There were improvements in gas analysis, especially in the determination of nitrogen, and a great variety of analytical separations. A new volumetric method for analyzing the salts of heavy metals was worked out, in which a metal such as copper or lead was precipitated as sulphide, the acid being afterwards determined by titration. The estimation of manganese as pyro-

* Ber. Deutsch. chem. Ges., 4,280. Not a separate paper, but part of his correspondence.

phosphate was another of these contributions to analysis. But the most important of all was the electrolytic determination of copper, now universally used, which was first published from Gibbs' laboratory. It is true that Luckow claimed to have used the method much earlier, but, so far as can be discovered, he failed to publish it. Gibbs, therefore, is entitled to full credit for a process which was the progenitor of many others. The entire field of electroanalysis was thrown open by him, and it has been most profitably cultivated. Gibbs also invented several instrumental devices of great convenience. The ring burner, and the use of porous septa when precipitates are to be heated in gases, are due to him. Furthermore, in cooperation with E. R. Taylor, he devised a glass and sand filter which was the forerunner of the porous cones invented by Munroe when the latter was a student in Gibbs' laboratory. That, in turn, preceded the well-known perforated crucibles of Gooch, who was one of Gibbs' assistants. The genealogy of these inventions is perfectly clear.

We now come to the remarkable series of researches upon the complex inorganic acids, which Gibbs began to publish in 1877, and continued well into the nineties. The ground had already been broken by others; silicotungstates, phosphotungstates, phosphomolybdates, etc., were fairly well known, but they were commonly regarded as exceptional compounds rather than as representatives of a very general class. In his first preliminary communication upon the subject Gibbs indicated the vastness of the field to be explored, and showed that the formation of complex acids was characteristic of tungsten and molybdenum to an extraordinary degree. The phenomena were general,

not special; and no limit could be assigned to the possible number of acids which these elements might form.

In his systematic work, following his preliminary announcement, Gibbs first revised the sodium tungstates in order to determine their true composition. Then, after preparing a number of phosphotungstates and phosphomolybdates, he studied the corresponding compounds containing arsenic in place of phosphorus. He next obtained similar vanadium compounds, and also showed that the phosphoric oxide of the first known acids was replaceable by phosphorous and hypo-phosphorous groups. Later still, he replaced the normal phosphates by pyro- and meta-phosphates, and also prepared complex salts containing arsenious, antimonious, and antimonic radicles. Stannophosphotungstates and molybdates, platinotungstates, and complex acids containing mixed groups were discovered, together with analogous compounds of selenium, tellurium, cerium, and uranium. One salt described, a phospho-vanadio-vanadico-tungstate of barium, had the formula

$$60\ WO_3.3P_2O_5.\ V_2O_5.VO_2.18BaO.150H_2O,$$

with a molecular weight of 20066. Compared with this substance the supposed complexity of most organic compounds is simplicity itself, and their interpretation seems relatively like child's play. In all, Gibbs described complex salts belonging to more than fifty distinct series.

In 1898, in his address as retiring President of the American Association for the Advancement of Science, Gibbs summed up his views as to the constitution of the complex acids. His presentation of the subject, however, can hardly be regarded as final. The problems involved

are too complicated to be easily solved, and much future investigation is needed in order to determine the true character of these extraordinary substances. Gibbs was a pioneer, breaking pathways into a tangled wilderness; but the ways are now open, and he who wills may follow. Possibly some of the compounds so far obtained were double salts; others may have been isomorphous mixtures; and in some instances phenomena of solid solution perhaps obscured the truth. By physical methods, cryoscopic or ebullioscopic, the molecular weights of the salts must be determined; their ionization needs to be studied, and in such ways their true nature can be ascertained. These methods of research have been mainly developed since the work of Gibbs was done; he, therefore, cannot be criticised for not employing them. Since his time chemists have come to recognize many compounds as salts containing complex ions, such as, for example, the oxalates, tartrates, etc., of iron, aluminium, chromium, and antimony, with other bases of lower valency. Even many of the silicates are easiest to interpret as salts of alumino-silicic acids, although the physical proof of their nature is difficult to obtain. The constitution of the complex acids is one of the great outstanding problems of inorganic chemistry.

Although he was distinctively an inorganic chemist, Gibbs did not entirely neglect organic chemistry. In 1868 he discussed the constitution of uric acid and its derivatives, and in 1869 he described some products formed by the action of alkaline nitrites upon them. He also produced several memoirs upon optical subjects, such as one upon a normal map of the solar spectrum, and another upon the wave lengths of the elementary spectral lines. Again, he

devoted some time to the study of interference phenomena, and discovered a constant, which he called the interferential constant, that was independent of temperature. One of Gibbs' latest papers, published when he was seventy-one years old, related to that extremely difficult subject, the separation of the rare earths—a subject in which he had always taken a deep interest. In this paper he developed a new method for determining the atomic weights of the rare-earth metals, which was based upon analyses of their oxalates. The oxalic acid was determined by titration with permanganate solutions, and the oxides by ignition of the salts. From the ratios between the oxalic acid and the oxides the molecular weights of the latter could be computed without reference to the amount of moisture in the initial substances. This method has since been employed by others, and especially by Brauner, in his work on the atomic weights of cerium and lanthanum. It is worth noting here that Gibbs had previously taken some part in atomic weight determinations. Those of Wing on cerium, and of Lee on cobalt and nickel, were made in Gibbs' laboratory and under his guidance. Furthermore, Gibbs was one of the earliest American chemists, if not the first, to accept the modern or Cannizzaro system of atomic weights, and to use it in his teaching.

Gibbs wrote no books and delivered no popular lectures. He was president of the National Academy of Sciences from 1895 to 1900, and he also presided over the American Association for the Advancement of Science in 1897. Honorary membership in the German, English, and American chemical societies, and in the Prussian Academy was conferred upon him.

His important contributions number 76.

In the universities of the Western States there have been many persons who have materially advanced the interests of chemistry. In this group, and really a pioneer in it, was Albert Benjamin Prescott, born in Hastings, N. Y. (1832). His early education was obtained from private tutors, from whose care he passed to the University of Michigan and there graduated from its medical department in 1864.

At the close of the war he returned to Ann Arbor to accept the place of assistant professor of chemistry and lecturer on organic chemistry; and five years later, he was made professor of organic and applied chemistry and of pharmacy. Meanwhile, in 1868, the School of Pharmacy was organized and charge of its instruction was at once given to Professor Prescott. Until 1880, the greater portion of the special and practical pharmaceutical instruction, including the laboratory work as well as the lectures, was given by him personally. During this period nearly 140 contributions of original investigations, representing work done by the students and graduates of this school, were published in various technical journals. These researches were made under the supervision of Prescott.

In 1876 he became Professor of Organic Chemistry, and the researches, conducted in the chemical laboratory under his direction, have been published, with the title of "Contributions from the Chemical Laboratory of the University of Michigan." During 1875-78 they appeared in the *American Chemist* and *Chemical News;* in 1880, in the *Journal of the American Chemical Society* and the *American Chemical Journal;* and, in 1883 and 1884, as separate

publications in association with Professor Victor C. Vaughan. They are octavo pamphlets, averaging fifty pages each.

While distinctly a chemist, the investigations of Prescott were naturally in the direction of the application of his chosen science to that of pharmacy, and much of the work executed under his supervision was published in the *American Journal of Pharmacy*. During 1876-78 portions of this work appeared with the title of "Contributions of the School of Pharmacy of the University of Michigan," but later they were published with separate titles.

He was very active in the work connected with the revision of the "Pharmacopœia of the United States." He served as a member of the revision committee in 1880, when he was made chairman of the sub-committee on descriptive chemistry, and furnished the assay methods for opium and cinchona, as well as the body of volumetric tests, which in that revision appeared for the first time. The general introduction of qualitative test limits, to fix the quantitative standards of medicinal purity of the chemicals of the Pharmacopœia, was first undertaken in this country by his sub-committee.

In connection with the revision of the Pharmacopœia in 1890 he showed considerable activity, and prepared an "Index of Contributions from the Michigan State Pharmaceutical Association, and the School of Pharmacy of the University of Michigan," to aid the national committee. It covered the time between the years 1883 and 1890, and included over ninety papers that represented work done under his supervision in the School of Pharmacy. Prescott was an active member of the Michigan State Pharma-

ceutical Association from its organization in 1883, and in 1886 contributed to its "Proceedings" an "Outline of a Plan of Study for the Assistant in Pharmacy," which has been extensively circulated in reprint form in response to a continuous demand for it.

Among his more popular contributions to scientific literature were papers on "The Material Resources of Life"; "The Aromatic Group in the Chemistry of Plants"; "The Chemistry of Coffee and Tea"; "The Chemistry of Fruit Ripening"; "Nostrums in Their Relation to Public Health"; and "Poisons and Their Antidotes." The foregoing are a few of the titles that appeared in *The Popular Science Monthly, Pharmaceutical Journal,* and Transactions, "Proceedings of the Michigan State Board of Health," and Wood's "Household Practice of Medicine." He wrote for other technical journals, such as the *London Chemical News* and the *Engineering and Mining Journal.*

A contemporary of his has well said: "His writings inspired respect for their author, for they were always important, thorough, and conclusive in their scope."

His text books are well known, and include "Qualitative Chemical Analysis," with Silas H. Douglas (Ann Arbor, 1874; fourth edition with Otis C. Johnson, New York, 1888); "Outlines of Proximate Organic Analysis" (New York, 1875); "Chemical Examination of Alcoholic Liquors" (1875); "First Book in Qualitative Chemistry" (1879); and "Organic Chemistry: A Manual of the Descriptive and Analytical Chemistry of Certain Carbon Compounds in Common Use" (1887).

The last named, which was his largest work, is un-

doubtedly the most complete and valuable book on the subject that has as yet been written by an American chemist.

He was a Fellow of the London Chemical Society and President of the American Chemical Society.

(Condensed from a more exhaustive biographical contribution by Dr. Marcus Benjamin.)

In the domain of agricultural chemistry, a pioneer and ardent student of the subject, whose influence has been most potent in the development of this branch of science was Samuel W. Johnson (1830-1909). He was born in New York State. His education was obtained in the Sheffield Scientific School of Yale University, at Leipsic and Munich in Germany. On his return to this country he became actively engaged as a member of the Yale faculty, where he held the chair of Theoretical and Agricultural Chemistry. He published a few articles on mineralogical subjects, and forty or more on pure chemistry, forms of apparatus and methods of analysis. His first publication was on "Fixing Ammonia" (1847). He contributed numerous articles to the *American Cultivator* (1854-1856). His paper on "Peat" (1859) is recognized as the best agricultural essay on this subject. His "How Crops Grow" (1868) has been more widely studied than any other work on agriculture. It was translated into Japanese, Italian, Russian, German, and Swedish. The supplementary volume "How Crops Feed" (1870) met with a similar hearty reception. In this connection his own words may be used: "My office has been to digest the cumbrous mass of evidence in which the truths of vegetable nutrition lie buried out of the reach of the ordinary inquirer and to set them forth

in proper order and in plain dress for their legitimate and sober uses."

Through his efforts the Connecticut Experiment Station —the first agricultural experiment station on the Continent —was established (1875). His addresses and papers before the State Agricultural Society and the State Board of Agriculture were of incalculable value and aid to the farmers. The reports of these societies became classic as an encyclopedia of agricultural science.

Johnson was president of the American Chemical Society (1878) and of the Association of American Agricultural Colleges and Experiment Stations (1895). He was a Fellow of the American Academy of Arts and Sciences (1866), and a member of the National Academy of Sciences.

John W. Mallet (1832-1912) was born near Dublin. He received his baccalaureate degree from the University of Dublin (Trinity College). In 1849 he published "A Chemical Examination of Killinite." His doctorate was obtained from the University of Goettingen (1852) with a thesis on the chemical examination of Celtic antiquities in the Museum of Dublin. In 1853 he came to the United States and for one term was professor of analytical chemistry in Amherst College. In 1854 he assumed the post of chemist to the Geological Survey of Alabama and the professorship of chemistry in the University of Alabama, where he continued until 1861. During these years he analyzed many rare minerals and communicated his results to the *American Journal of Science.* His published contributions number more than one hundred, notable among which are: "A Determination of the Atomic Weight of Lithium"

(1856), "The Atomic Weight of Aluminium" (1857, 1880), and a revision of the Atomic Weight of Gold (1889).

"In May, 1862, Mallet then, as always, a British subject, was given general supervision of the manufacture of ammunition for the Southern Confederacy, in which capacity he was most actively engaged throughout the war."

In the fall of 1865 he was professor in Tulane University. In 1868 he took up similar duties at the University of Virginia. In 1883 he changed to the University of Texas. The following year he held a similar appointment at the Jefferson Medical College, Philadelphia, but the next session returned to the University of Virginia, where he remained until his death.

"Long is the roll of those who will ever recall with thankfulness the privilege of sitting under his teaching."

He was the recipient of many honors.

Chemists of America are justly proud of the splendid researches of M. Carey Lea (1823-1897). Working quietly and modestly, he made a marked impress upon the science; indeed, in the domain of photochemistry he was a true pioneer. His numerous studies, both chemical and physical, have been sympathetically and exhaustively reviewed by Dr. G. F. Barker in *Biographical Memoirs of the National Academy of Sciences,* Vol. 5, 157. From this fascinating brochure the following paragraphs have been extracted:

In his paper "On Numerical Relations existing between the Equivalent Numbers of Elementary Bodies," Lea undertook to show that the number 44.45 plays an important part in the science of stoichiometry, and that the relations which depend upon it are supported, in some

cases at least, in a remarkable manner, by analogies of atomic volume.

His numerical computations show that that relation spoken of extends to no less than forty-eight of the elementary bodies.

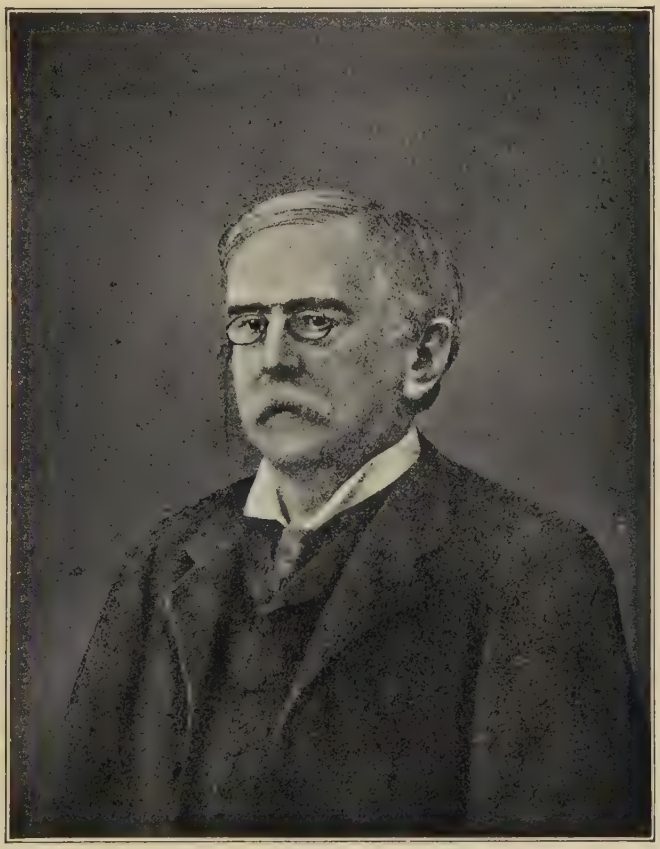
In the second part of this paper a new and wholly distinct relation is pointed out, which Lea called a relation of "geometrical ratios" to distinguish it from the relation of arithmetical differences. Their nature consists in this: "that if we take two substances and examine the ratio which subsists between the numbers representing their atomic weights, we may find in certain cases that it is identical with the ratio subsisting between the atomic weights of two other substances."

In 1864, he published two papers on the Platinum group entitled (1) "Notes on the Platinum Metals and Their Separation from Each Other," and (2) "On Reactions of Platinum Metals."

The use of oxalic acid for purifying the double chloride of iridium and ammonium was here proposed for the first time and possessed marked advantages over the older methods. It also calls attention to a new reaction for ruthenium: when a solution of hyposulphite of soda is mixed with ammonia and a few drops of sesquichloride of ruthenium solution is added, a magnificent red-purple liquid is produced, which, unless quite dilute, is black by transmitted light. The chief value of this test for ruthenium lies in the fact that it is capable of detecting ruthenium in presence of an excess of iridium.

In 1866 Lea called attention to the great increase of delicacy which is produced in the starch reaction for iodine by adding chromic acid to the solution.

In 1874, he described a new compound formed by the union of mercuric iodide, with silver chloride, analogous to the double iodide of mercury and silver, and that of mercury and copper, which had already been obtained by



M. Carey Lea

Meusel. The substance exhibits remarkable properties in its relations to heat. Even below 100° it begins to redden, and the color increases up to about 140°, when it has a bright scarlet color, resembling vermilion. On cooling, its natural color returns.

In a paper "On the Nature of Certain Solutions and on a New Means of Investigating Them" (1893) he pointed out that two classes of salts are formed by the three best known acids; the one perfectly natural, like the alkali salts, the other decomposed by water like mercuric sulphate, bismuth nitrate, and stannous chloride.

From an extended investigation of the conditions, the author concludes:

(1) that the solution of iodo-quinine affords the means of connecting free sulphuric acid even in traces, in presence of combined sulphuric acid; (2) that the salts of protoxides of the heavy metals do not owe their acid reaction to dissociation. With one exception, the solutions of their sulphates contain no free sulphuric acid. This exception is ferrous sulphate, whose solutions always contain free acid; (3) that sesquisulphates are always dissociated in solution; (4) that alums, with the exception of chrome alum, are always dissociated in solution; and (5) that acid salts are dissociated in solution; sometimes perhaps completely.

Another noteworthy paper of the following year (1894) was a "New Method of Determining the Relative Affinities of Certain Acids." This method was based on the principle that "the affinity of any acid is proportionate to the amount of base which it can retain in the presence of a strong acid selected as a standard of comparison for all acids." This contribution contains much that is of value at the present time.

The same year (1894) he proposed two new methods for reducing platinic to platinous chloride—one by the action of potassium acid sulphite, the other by that of alkali hypophosphites.

In organic chemistry, Lea also made many valuable investigations. He published very early a series of papers on picric acid and its compounds, giving in the first of these, which appeared in 1858, an important modification of one of the processes for preparing the acid.

In a second paper he discussed the claim made for picric acid as a test for potash.

In 1861 he gave a résumé of his observations on picric acid, considering its solubility in sulphuric acid, the tests for it, the methods of its purification, and the effect of reducing agents upon it.

Subsequently Lea made a number of studies on the ethyl and methyl bases. For preparing ethylamine he advises to mix nitrate of ethyl with its own volume of strong alcohol, and to add an equal bulk of a concentrated solution of ammonia. On placing the liquid in strong tubes and heating on the water bath to boiling for three hours, the reaction is completed, diethylamine and triethylamine being formed at the same time. To separate these bases from each other they were converted into picrates, the triethylamine salt being extremely insoluble, the salt of diethylamine extremely soluble, and the solubility of ethylamine picrate intermediate.

He afterwards prepared methyl bases by heating together strong ammonia and methyl nitrate, as in the case of the ethyl bases. The chief product was methylamine. For the preparation of the methyl nitrate he found the use of urea, dissolved in the methyl alcohol, a most satisfactory modification.

In obtaining urea from ferrocyanide of potassium, Lea effected a more complete oxidation by the use of a larger quantity of red lead. He obtained as much as 500 grams of urea from 850 grams ferrocyanide.

In 1861, while preparing naphthylamine by the reduction of nitronaphthalin, he observed that if heat be applied before adding the caustic alkali a distillate is obtained

which has a pale reddish color and which possessed the disgusting odor of naphthylamine. Mineral acids change its color to pale violet. Heated with sulphuric acid it becomes a rich blue-purple and deposits after a time a small quantity of a black crystalline precipitate. For this new coloring matter he proposed the name ionaphthine.

Subsequently he described another colored derivative of naphthalin, obtained in the course of preparation of the chloride by passing chlorine over it.

In a paper published in 1865 he calls attention to a new reaction of gelatine, the first ever described as produced between pure gelatine and a perfectly colorless reagent. When a piece of gelatine is dropped into a solution of pernitrate of mercury it gradually assumes a strong red coloration and after a time dissolves completely to a fine red solution. On boiling for some minutes, its color deepens; but it is quickly decolorized by chlorate of potash. Metagelatine, prepared by allowing gelatine to swell in a cold saturated solution of oxalic acid, then heating moderately until the mass remained fluid on cooling, and removing the oxalic acid by carbonate of lime, was found to give the red coloration even more decidedly than ordinary gelatine.

In a paper on the detection of hydrocyanic acid Lea gives the test. If a pure protosalt of iron, such as ferrous-ammonium sulphate, mixed with a little uranic nitrate, be dissolved in water, the solution gives, with a soluble cyanide, a purple precipitate, which in very dilute solutions is grayish purple. The solution should be quite neutral and nearly colorless.

Besides his purely chemical papers, he published many others in the domain of physics. As early as 1860 he called attention to the optical properties of picrate of manganese, and made an extended study upon it.

In 1861 Sprengel had devised an air blast for laboratory use founded on the well-known Catalan trompe (*Am.*

Jour. Sci., II, xxxii, 425). The following year Lea conceived that the principle might be made use of for aspirating as well as for blowing, and he described an apparatus performing both functions simultaneously, and admirably adapted to the purpose. This apparatus, in the form of the Bunsen filter pump, has since come into general use, especially for laboratory purposes; and, as modified and improved by Crookes, it has made possible to science the high vacua which he has so thoroughly studied, and to electric lighting the construction of the incandescent lamp.

Lea (1869), having observed that when a beam of sunlight is thrown upon a white screen at fifteen or twenty feet distance, and a plate of finely ground glass is interposed, the white light acquires a deep orange yellow color, set himself to investigate the phenomenon. Three aspects of it were observed: First, where a strong beam of yellow, red, or reddish yellow direct light is produced without the complementary blue being visible and simultaneously the blue, the latter diffused; and third, where reddish and bluish light, both diffused, are simultaneously visible. He concludes that all these results are due to interference, and are capable of satisfactory explanation upon this hypothesis.

By far the most valuable as well as the most extended investigations of Lea, however, were those which related to the chemistry of photography, in which at the time of his death he was the acknowledged authority. His researches were directed chiefly to the chemical and physical properties of the silver halide salts, not only alone, but also in combination with each other and with various coloring matters, especially with reference to the action of light upon them under all these different conditions.

In one of his papers (1865) he gave a series of experiments which seemed to him to decisively close the controversy then in progress in favor of the physical theory—

i. e., the theory that the change which takes place in an iodobromized plate in the camera is a purely physical one; that no chemical decomposition takes place, and hence that neither liberation of iodine nor reduction of silver results.

In 1866, he prepared a résumé of a series of investigations whose object was to fix with greater exactness the obscure chemical and physical phenomena which lie at the basis of the photographic art, the details of which had been published in the photographic journals. While it may be conceded that silver chloride and bromide undergo reduction while exposed to light, opinions differ as to the iodide. Pure silver iodide, he maintained, when exposed to light, received a physical impression only; but if certain other substances, such as silver nitrate, tannin, etc., are present, then a chemical action, a reduction, may take place. In proof of the first statement a glass plate surrounding a film of pure silver iodide was exposed to sunlight for many hours, then enclosed in a dark closet for thirty-six hours, then placed under a negative and exposed to light for two seconds. On pouring a developer over it a clear, bright picture instantly appeared. Hence the action of the sun for many hours had produced an impression which disappeared completely in thirty-six hours. If the action of light was to reduce the iodide in sub-iodide, how does this sub-iodide receive its lost proportion of iodine? The fact that the iodide was much more powerfully affected by a recent exposure of two seconds than by one which, though thirty-six hours old, was only a thousand times as strong, and in light much more intense, seemed fatal to the chemical theory. By means of other direct and indirect experimental evidence he concluded that the action of light upon pure isolated silver iodide cannot be a chemical reduction.

In a paper on "Contributions Toward a Theory of Photo-Chemistry," published in 1867, Lea developed a re-

markably ingenious theory based on the phenomena observed in silver iodide, which are the key to the whole subject. When this iodide is exposed to light it acquires a new property, that of attracting to itself a metallic precipitate in the act of forming, or a metallic vapor ready formed. A film of this iodide exposed for many hours to a bright sun does not further darken beyond the change produced by the first instance of diffuse light; and then if put in the dark for a brief time it re-acquires the capacity to receive an image by exposure for a second. What, then, is the nature of this change—this impression received in a second and then slowly passing spontaneously away? Lea found the answer in the phenomena of phosphorescence. When silver chloride is exposed to light it becomes violet in color, losing one-half its chloride—i. e., it is decomposed; but when silver iodide is thus exposed no chemical change takes place, but the impression is for a time persistent. The "physical impression of light is a persistence of the invisible (or chemical) rays exactly parallel to the persistence of visible or luminous rays in phosphorescence." For this function of light, the existence of which produces the physical change suffered by exposed silver iodide, Lea proposed the term actinescence. Just as in the case of phosphorescence, a body temporarily retains light and subsequently emits it, this emission being rendered evident by luminous phenomena, so by actinescence we have the phenomena of the storing up of light, where certain objects exposed to light and then carried into darkness have acquired the power of acting chemically upon other bodies with which they were placed in contact.

In a paper in 1868 Lea pointed out the well-known fact that silver bromide, when exposed to light, undergoes decomposition, with elimination either of bromine or a bromine compound, being at the same time reduced to subbromide, the result being a distinct darkening. In his experiments organic matter was eliminated by forming the

film on a plate of glass, first by silvering it, and then treating it with bromine or iodine. On exposure for four hours the pure and isolated bromide film gave a distinct impression. Since silver iodide is not thus decomposed, the latent image produced by its exposure to light must be purely physical. The object of the paper was to show that silver bromide is also capable of forming a latent image, in which chemical decomposition plays no part, and which therefore must be distinguished from a chemical image. This physical image, however, is quite different from that formed on silver iodide. While on the latter the physical image is produced only when the iodide is isolated from all other bodies, that on the bromide is found only in the presence of organic matter; and, secondly, while the physical image on silver iodide can be called forth only in the presence of silver or of some other metallic body, this image on silver bromide can be developed in the complete absence of any metallic body. Let a collodion film containing silver bromide and free silver nitrate be formed on glass, washed, plunged into a solution of tannin, and dried. Expose this for a short time in the camera. A latent image is formed. Place the plate in pyrogallol solution and the image appears. But how is this? It cannot be that an infinitesimal chemical image of sub-bromide, acting as a nucleus, was brought up to a visible intensity by the action of the developer, because pyrogallol alone has no power to do this, and because free silver nitrate must also be present, and this had been removed in the washing. The only alternative is that that portion of the film upon which the light had acted was so modified thereby that it was brought into a condition to be more easily decomposed by pyrogallol than the portion which had not been acted on. Now, if portions of the bromide film not decomposed by light, but simply acted on by it, are subsequently decomposed by the action of pyrogallol, while those portions of the film not influenced by light are not decomposed by

the pyrogallol, then it follows that the action of the light is so far simply physical.

In 1878 Lea sought to determine the precise amount of material actually altered by the action of light. Silver chloride, precipitated with excess of hydrochloric acid, and well washed, was exposed to bright sunlight for five days. Two grams of the dark powder were thoroughly treated with sodium hyposulphite to remove the unaltered chloride. The residue weighed twenty-one milligrams, showing that only about 1 per cent. of the chloride had been acted on. If it be assumed that the action consists in the removal of one-half the chlorine, the whole loss in weight by the action of the light would be only a little more than one-tenth of 1 per cent. As the best reagents for removing the unaltered chloride are liquid ammonia and sodium hyposulphite, and as both of them attack the altered substance, the difficulty in verifying the nature of the action of light is very considerable. Nitric acid does not attack the darkened portion of the silver chloride in the least, while the dark residue left by the above reagents is instantly dissolved by cold nitric acid with evolution of red fumes. Evidently no metallic silver is produced by the action of the light, while it is produced by the subsequent action of the ammonia and the hyposulphite. Since the black substance is made white by aqua regia, it evidently contains less chlorine than the chloride, and so may be either a subchloride or an oxychloride. The substance produced by the action of light on silver chloride is of a much more permanent character than in the case of the other silver halides.

When silver iodide is blackened under ammonia solution in a test-tube and set aside in the dark for a day or two, the iodide assumes a singular pinkish shade. Hence it appears that silver iodide, under the influence of ammonia and of light, gives indications of most of the colors of the spectrum. Starting with white, it passes under the

influence of light to violet and thence nearly to black. This violet-black substance, washed with water, passes to brown. The brown body, when covered with ammonia and left to itself in an open-test tube, becomes pinkish in the dark and yellow in sunlight. These curious relations to color observed in the silver halides seem to give hope of the eventful discovery of some complete method of heliochromy.

In 1874 Lea made an extended series of experiments to test a theory advanced shortly before by Vogel, to the effect that substances placed in contact with silver bromide modify its impressibility by rays of different refrangibilities. His results seemed to establish the fact that there is no general law connecting the color of a substance with the greater or less sensitiveness which it brings to any silver halide for any particular ray.

In 1875 an elaborate investigation was made on the action of the less refrangible rays of light on silver iodide and bromide. Experiments were also made to test the theory that light consists of two classes of rays, the "exciting" and the "continuing" rays. The conclusions reached as a consequence of the one hundred and sixty experiments made were:

1. Silver bromide and iodide are sensitive to all the visible rays of the spectrum.

2. Silver iodide is more sensitive than the bromide to all the less refrangible rays, and also to white light.

3. The sensitiveness of silver bromide to the green rays is materially increased by the presence of free silver nitrate.

4. Silver bromide and silver iodide together are more sensitive to both the green and the red rays (and probably to all rays) than either the bromide or the iodide separately.

5. There do not exist any rays with a special exciting or a special continuing power, but all the colored rays are

capable both of commencing and continuing the impression on silver iodide and bromide.

In a research published in 1876, Lea confined himself to the question: Does there exist any red substance which is capable of increasing the sensitiveness of silver bromide to the green rays? The result was that not a single red substance could be found that possessed that property, while no less than eight colorless substances exhibited it. Hence the conclusion: "There exists no relation between the color of a substance and that of the rays to which it increased the sensitiveness of silver bromide."

In 1877, he pointed out that salts of silver may exhibit sensitiveness to light in three ways: They may exhibit a sensible darkening or they may receive a latent image; and this may have a capacity of being rendered visible either by receiving a deposit of metallic silver or by decomposition by alkalies in connection with reducing agents.

Since the development of the latent or invisible photographic image produced by the action of light is, beyond all question, the most remarkable and the most interesting fact in photochemistry, it is surprising with what slowness our knowledge of the substances capable of producing this effect has increased. In 1877 he further made an extended examination of substances likely to act as developers. The results show (1) that the number of bodies endowed with the power of developing the latent image, so far from being very limited, as hitherto supposed, is, on the contrary, very large; and (2) that potash acts more powerfully in aiding development than ammonia, contrary to the general opinion. Moreover, he observed that the use of free alkali is not necessary to the most energetic development, as has been supposed; and this led him to devise a form of development which, though there is no free alkali present, is more powerful than any yet known. Among the various developing substances examined, the salts of ferrous oxide proved to be the most interesting and remark-

able of all in their action on the image; and of these, ferrous oxalate exhibited developing powers of a very marked kind. The same exposure which with alkaline pyrogallol gives a weak and sunk-in image after a protracted development gave with ferrous oxalate a bright, bold image and in much less time. The development was particularly clear and clean. The unexposed parts were not attacked. The developer possessed a great deal of that elective power previously mentioned, which caused it to react strongly on those parts which received the influence of light and to spare those which have not. Three years later the study of the developing power of ferrous salts was resumed and active ferrous salts were found to be the borate, phosphate, sulphite, and oxalate.

The fact is well known that certain organic substances, tannin, for example, placed in contact with the washed halide, increase its sensitiveness. Poitevin and Vogel proposed the theory that these substances acted in virtue of their affinity for the halogen. But Carey Lea pointed out soon afterward that the one property that these substances possess in common is that they are all reducing agents. Hence he concludes that their action is due to the fact that they abstract the halogen, but that their affinity comes in to aid the affinity of the halogen for the hydrogen, and that under the influence of the light water is decomposed. According to this view, whenever silver iodide is exposed to light in presence of an organic body capable of accelerating the action of this agent, there should be formed traces of free acid; whereas the Poitevin-Vogel theory requires the formation of an iodo-substitution compound. To test the question, silver iodide precipitated with excess of potassium iodide was well washed, and exposed, after receiving a small quantity of pyrogallol, to sunlight for fifteen minutes in presence of water. The liquid, which was at first neutral, showed a distinct acid reaction at the end of fifteen minutes. Again, on the Poitevin-Vogel theory, a

substance having an affinity for iodine should increase the sensitiveness, and substances not having it should have no such action. But this is not the fact. Substances like potassium carbonate solution and like starch, for example, which have an affinity for iodine, do not appear to increase the sensitiveness of silver iodide by contact with it. Hence he concludes that such organic bodies as increase the sensitiveness of the silver halides to light do so not by forming substitution compounds with the halogen, but by promoting, in virtue of their affinity for oxygen, the decomposition of water by this halogen.

As early as 1866 Lea had proved that the intense black substance produced by the action of light upon silver iodide in presence of silver nitrate contains iodine, and therefore is either a sub-iodide or an oxy-iodide. In 1874 he extended his examination to silver bromide. He observed that when silver bromide is treated with pyrogallol and alkali after exposure to light, the black substance contains bromine, and is resolved by nitric acid into normal silver bromide and silver, which latter is dissolved. It is therefore either a sub-bromide or an oxy-bromide, probably the former. In 1878 he proved the same fact for the black substance given by the chloride when acted on by light.

Perhaps his most important contribution to photochemistry was his discovery of the photosalts. His earliest paper on the subject appeared in 1885, and described the remarkable property possessed by the silver halides of entering into chemical combination with certain coloring matters in somewhat the same way as alumina does, forming lakes. The freshly precipitated and still moist silver salt is brought into contact with the coloring matter, or it is precipitated in presence of it. The union takes place readily, and the color cannot be washed out. What is curious and seems to be evidence that the combination is intimate is the fact that the color assumed by the silver salt may differ considerably from that of the dye. The

three halides may even be differently colored by the same coloring substance. Generally, however, coloring matters impart their own shade or something like it to the halide. The bromide precipitated in presence of silver nitrate takes from aniline purple a strong purple color, from cardinal red a bright flesh or salmon color, from naphthalin yellow a yellow color. Sometimes different specimens of the same coloring matter give different colors. Silver bromide received from one specimen of methyl green a bluish green, while another specimen of the same dye produced in it a deep purplish color. As early as 1868 he had proposed * to color or stain the photographic film in order to modify its behavior toward light—i. e., to prevent blurring or irradiation. At that time the best color was found to be red litmus. The theory of Vogel that a film thus colored gained sensitiveness to those rays of the spectrum which the coloring matter absorbs is made improbable by the fact that the color in the film tends to arrest precisely those rays to which it is proposed to render the silver salt more sensitive. John W. Draper's view that substances sensitive to light are affected by the rays which they absorb seems a priori more probable. Lea, however, observed that the effect will depend, first, upon the capacity of the dye to combine with the silver halide, and, second, not on the proper color of the isolated dye, but on the color that the silver halide acquires from it.

The paper containing his photosalt theory was published in 1887. Of the two theories of the latent photographic image, the physical and the chemical one, he had inclined at the outset to the first of these. Of late years results have been obtained not readily reconcilable with it. On the other hand, the theory that the latent image is formed of subsalts is open to the objection that while subsalts are readily attacked by nitric acid, the latent image may be exposed without effect to this acid. Three years

* *British Journal of Photography*, xv, 210, 506, 1868.

of laboratory work had led him to a truer theory, based on the fact that silver halides are capable of uniting with coloring matters to form stable compounds. He now found that in much the same way a silver halide may unite with a certain proportion of its own subsalt to form colored compounds, which by this union loses its characteristic instability and yields a compound of great permanence. When silver chloride, bromide, or iodide contains as little as one-half of one per cent. of subsalt combined with it, its properties are greatly changed. It has a strong coloration and its behavior to light is altered. It is one of the forms of this substance which constitutes the actual material of the latent photographic image. Of the three, the chlorine salt is the most stable and exhibits the finer variety of coloration. Hence it is the most interesting because of its relations to heliochromy. It shows all the warm shades from white to black through the following graduations; white, pale flesh color, pale pink, rose color, copper color, red purple, dark chocolate, black. These compounds are obtained in an endless variety of ways—by chlorizing metallic silver; by acting on normal chloride with reducing agents; by partly reducing silver oxide or silver carbonate by heat and treating with hydrochloric acid or silver carbonate by heat and treating with hydrochloric acid, followed by nitric acid; by acting on subchloride with nitric acid or an alkaline hypochlorite; by attacking almost any soluble salt of silver with ferrous, manganous, or chromous oxide, followed by hydrochloric acid; by treating a soluble salt or almost any silver solution with potash or soda and almost any reducing agent—cane sugar, milk sugar, glucose, dextrine, aldehyde, alcohol, etc.—and supersaturating it with hydrochloric acid. So, also, almost any salt of silver exposed to light, treated with hydrochloric acid and then with strong nitric acid yields it. Since these substances have been seen hitherto only in the impure form in which they are produced by the continued action of

light on the normal salts, Lea proposed to call them photosalts; as photochloride, photobromide, and photoiodide.

In the second part of his paper he proved that the strongly colored photosalts, obtained independently of any action of light, are identical, first, with the product obtained by the continued action of light on these halides, and, second, with the substance of the latent image itself. If silver chloride precipitated with excess of hydrochloric acid be exposed to light, we get a deep, purple-black substance which, when boiled with dilute nitric acid, gives up a little silver and becomes a little lighter, changing to a dull purple, resembling closely some of the forms of photochloride already described, chiefly those produced by the action of sodium hypochlorite, or of ferric chloride on metallic silver. When silver oxalate is covered with water and exposed to sunshine for two days, being frequently agitated, it changes to a deep brownish black, becoming a little lighter by treatment with hydrochloric acid. When washed and boiled with nitric acid it acquires a fine deep copper red color. A sample especially prepared in this way which had a fine lilac purple color was found to contain one half of 1 per cent. of subchloride. The red chloride thus obtained by the action of light on silver oxalate not only resembles closely the red chloride obtained by means exclusively chemical, but shows the same behavior to reagents. In considering the question of the latent image the author called attention to the remarkable fact that a dilute solution of sodium hypophosphite if poured over a mass of silver chloride, bromide, or iodide had the property of bringing those substances into the condition in which they exist in the latent image. Applied in strong solution with the aid of heat it produced brown photochloride, photobromide, or photoiodide of silver. Experimental evidence is given to show, first, that in the entire absence of light sodium hypophosphite is able to affect a sensitive film of silver halide exactly in the same way as does light, produc-

ing a result equivalent to a latent image formed by light and capable of development in the same way as an actual impression of light; second, that these two effects, the impression produced by hypophosphite and that by light, comport themselves to reagents exactly in the same way, and seem in every way identical; and, third, that the image produced by hypophosphite on silver chloride always gives rise to a positive development, but on silver bromide may give rise either to a direct or to a reverse image, both of these effects corresponding exactly with those of light. But more than this. Sodium hypophosphite may be made to reverse the image produced by light on silver bromide, and conversely, light may be made to reverse the action of hypophosphite. It would seem, therefore, that the question of the identity of the photosalts with the products of light on the silver halides may, perhaps, with some confidence be allowed to rest on the cumulative proofs here offered.

As long ago as 1878 Lea had shown that as the black substance produced by the action of light upon silver chloride became white on treating with aqua regia, it evidently contained less chlorine than the chloride, and so must be either a subchloride or an oxychloride. He succeeded in demonstrating it to be a compound of a normal salt with a sub-salt.

In further proof of the existence of hemi-compounds, Lea, in 1892, obtained a double salt of hemisulphate, and normal sulphate containing one molecule of each.

Perhaps the most remarkable discovery made by Lea was that of allotropic silver. In 1886 he had taken up the study of the reduction of silver in connection with that of the photosalts. At first the results were most enigmatical, but eventually stable products, capable of a fair amount of purification, were obtained. The reaction employed was the reduction of silver citrate by ferrous citrate. Even the earlier and less pure forms of allotropic silver thus prepared were exceedingly beautiful; the purer are hardly sur-

passed in this respect by any known chemical products. The forms obtained he classified as: A, soluble, deep red in solution, mat-lilac, blue, or green while moist, brilliant green, metallic when dry; B, insoluble, derived from A, dark reddish brown while moist, when dry exactly resembling metallic gold in burnished lumps. Of this form there is a variety which is copper-colored. The C form is insoluble in water and appears to have no corresponding soluble form.

Lea subsequently pointed out that the three forms of allotropic silver are not to be understood as the only forms which exist, but only as the most marked.

In April, 1891, he wrote "That silver may exist in three forms: First, allotropic silver, which is protean in its nature; may be soluble or insoluble in water, may be yellow, red, blue, or green, or may have almost any color, but in all its soluble varieties always exhibits plasticity—that is, if brushed in a pasty state upon a smooth surface its particles dry in optical contact and with a brilliant metallic luster; it is chemically active; second, the intermediate form, which may be yellow or green, always shows metallic luster, but is never plastic, and is almost as indifferent chemically as white silver; third, ordinary silver." Further he pointed out "that allotropic silver can always be converted either into the intermediate form or directly into ordinary silver; that the intermediate forms can always be converted into ordinary silver, but that these processes can never be reversed; so that to pass from ordinary silver to allotropic it must first be rendered atomic by combination, and then be brought back to the metallic form under conditions which check the atoms in uniting; that allotropic silver is affected by all forms of energy, and that this effect is always in one direction, namely, toward condensation; that the silver halides are similarly affected by the same agencies; that a remarkable parallelism is noticeable between the two actions, especially if we take into

account the fact that in the halides the influence of energy is to some extent restrained by the strong affinity which the halogens show for atomic silver. There is therefore reasonable ground to suppose that the silver halides may exist in the allotropic form."

In the course of his investigations Lea became greatly interested in the relations of energy to the chemical changes in matter. Since it is well known that when a substance is capable of existing in two allotropic forms and being converted from one into the other by pressure, the body resulting from pressure is always the more dense of the two, is less active chemically, and is a polymer of the first, it should follow that allotropic silver, which is converted into normal silver by the simple pressure of the finger, should be less dense than it and should have a greater chemical activity. This Lea demonstrated to be the fact. In the case of the three forms of silver—the allotropic, the intermediate, and the ordinary form—he showed as early as 1891 that while the first form can be converted into the second and third in several ways and with the utmost facility, and that the second can also be converted into the third, these transformations can by no possibility be reversed. To convert ordinary into allotropic silver we must as a first step dissolve it in an acid—that is, convert it from a polymerized into an atomic form—since only from this atomic form can allotropic silver be obtained. Hence he suggests that the three forms of silver may be considered as atomic, molecular and polymerized. Special experiments made upon the silver halides showed that these compounds, though substances of very great stability, have their equilibrium so balanced as to respond to the slightest influence, not merely of light, but of any form of energy; not receiving a momentary but a permanent impression, which, though so slight as to be invisible, still greatly increases the tendency of the molecule to fall in pieces under the action of a reducing agent. It is not light only, therefore, that is

capable of producing an invisible image. The power belongs alike to all forms of energy. In a paper read before the National Academy in April, 1892, he showed that not only heat, light, electricity, and chemism are capable of disrupting the molecule, but that mechanical force also is able to do this. Silver chloride was enclosed in platinum foil and exposed to a pressure of about one hundred thousand pounds to the square inch, maintained for twenty-four hours. The chloride was completely blackened except at its edges, where, of course, the pressure was less. Silver bromide gave the same result. Silver iodide was not blackened by light; but to his great surprise, it darkened under pressure to the same extent as the others. Even shearing stress obtained by simple trituration in a porcelain mortar produced a darkening of silver chloride—a true silver photochloride. These observations prove the existence of a perfect uniformity in the action of all kinds of energy on the silver halides. The balance of the molecule is at once affected by the influence of any form of energy. A slight application produces an effect which, though invisible to the eye, is instantly made evident by the application of a reducing agent. The bonds which unite the atoms have evidently been loosened in some way, so that these molecules break up more easily than those to which energy has not been applied. Hence, if the substance be submitted to the action of light, heat, or electricity, or if lines are drawn across it with a glass rod or with sulphuric acid, a reducing agent blackens the parts so treated before it affects the parts not so treated. Obviously the phenomena of the latent image and of its development are not especially connected with light, but belong to other forms of energy as well. It follows, therefore, that every form of energy is capable, not only of producing an invisible image —that is, of loosening the bonds which unite the atoms— but also, if applied more strongly, of totally disrupting the molecule. Mechanical force, even, is therefore competent

without the aid of heat to break up a molecule which owes its existence to an exothermic reaction. Obviously this phenomenon has nothing in common with decomposition produced by mechanical force in silver or mercury fulminate and similar explosives. Such substances are all formed by endothermic reactions, and their decompositions are exothermic. But silver halides are formed by exothermic reactions, and consequently their decompositions are endothermic and require the energy which was set free in their formation to be returned to effect their decomposition. The experiments now described show that mechanical force may be made to supply this energy, and so play the part of light, electricity, or heat without previous conversion into any of these forms.

In three papers on "Endothermic Reactions Effected by Mechanical Force," published in 1893, Lea generalized his proposition and sought to determine whether mechanical force would not be capable of bringing about analogous chemical changes in other compounds. This he showed was the case.

From his results he found first, a new classification of the elements based on more correct principles than those previously made use of, and, second, a proof that the color or non-color of an element is a function of its atomic weight. Considering the elements numerically, it appears (1) that those whose atomic weights are less than 47 have colorless ions only; (2) that colored ions suddenly commence with titanium (48) and form an unbroken series of eight elements up to copper (63.4); (3) that a series of nine metals follow having colorless ions only, beginning with zinc (64.9) and ending with yttrium (92.5); (4) that next come six metals with colored ions extending from columbium (94) to silver (107.7); (5) that these are followed by nine metals having colorless ions, from cadmium (111.6) to lanthanum (139); (6) that next come ten metals having colored ions, from cerium (142) to gold

(196.2); (7) that the six remaining metals are alternately colorless and colored, mercury (199.8) being colorless, thallium (203.6) colored, lead (206.4) colorless, bismuth (210) colored. From the conception of the all-important nature of the color of the atom, while that of the element is of little significance, the author drew several interesting conclusions: First, that the well-known Periodic law must be rejected as based on erroneous principles; and, second, that no element having ions colored at all valencies can belong to the same natural group with elements having colorless ions only. This law, which he calls the Law of Color, is rigorous and fundamental; rigorous because it admits of no exception; fundamental, because it divides elements into two chief divisions, with strongly marked differences.

The paper concludes with a discussion of the periodicity of the law of color, illustrated by a plate, commencing with hydrogen and showing a double series of eighteen elements, with increasing atomic weights, all having colorless ions only. Approaching the first of the colored groups—i. e., the iron group—we find the transitional elements titanium and vanadium, which have both colorless and colored ions, the former uniting them to the preceding and the latter to the following series. This alteration is continued through the list of elements, showing that with atomic weights from 1 to 47, from 65 to 90, and from 112 to 139 their atoms are colorless; from 52 to 59, from 103 to 106, from 145 to 149, and from 192 to 196 the atoms are always colored. Elements whose place in the numerical series falls between these periods have both colored and colorless atoms. The six remaining periods have both colorless and colored atoms alternately. Evidently the conclusion drawn by the author from these facts, "that the color of the elementary atoms is to a large extent a function of their atomic weights," is fully justified.

In his second paper he considers more in detail certain consequences of his general theory. The law of the inter-

action of ions he states thus: "If a colored substance be formed by the union of a colorless kation with a colorless anion, the color belongs to the molecule only. The colorless ions have so modified each other's vibration periods that selective absorption is exercised. As soon therefore as the molecule is divided into ions the color must disappear; consequently if we find a solvent, which, like water, is capable of separating the ions, the resulting solution when dilute must be colorless, no matter how intense the color of the compound." The truth of this law he experimentally tested, and found the results confirmatory without exception. With regard to the combination of ions, he states as follows: A, two or more similar colorless ions may unite to form a colored elementary molecule; B, two or more similar ions, colored, may unite to form a colorless (or white) molecule or polymer; C, two or more similar colored ions may unite to form a molecule of a wholly different color; D, two or more dissimilar colorless ions may unite to form a colored molecule. No ion, and therefore no atom, is black, but is always transparent to some portion or portions of the visible rays; atoms and ions differing absolutely in this respect from molecules. In considering the theory of the action of acid indicators he maintains that dissociation has no essential connection with their reactions. The fact simply is that by combining with alkalies these substances have their color much intensified or change it altogether. From the results of his color investigations Lea drew the following general conclusions: (1) When highly colored inorganic substances are composed of colorless ions, then if these substances can be brought into solution as electrolytes, the color wholly disappears. (2) The union of ions, colored and colorless, gives rise to the most surprising changes of color. (3) The change of color of an acid indicator placed in contact with an alkali in no way depends upon dissociation. (4) Selective absorption of the visual rays by an element can never constitute a basis for

classification, but the relation of ions to the visual rays leads to a classification which is in absolute harmony with the chemical characteristics of the elements. (5) While there is good reason for believing that in solution the ions are separated so as no longer to affect each other's vibrations, it is also certain that they remain within each other's range of influence, so that they cannot be considered as free.

Lea was universally regarded as the pioneer investigator in the more scientific realms of art, and his studies on such subjects as the preparation of collodio-bromide and emulsions, on the chemistry of developing agents, on the influence of color on the reduction by light of silver salts, especially the halides, and particularly his work on the remarkable tendency of these substances to form colored compounds practically of all possible hues, and so foreshadowing the success of heliochromy in a not remote future —these studies must ever be considered the most valuable contributions to the science of photography made during the last quarter of a century in which he lived.

Among the teachers of chemistry as well as among the investigators, a high place must be ascribed to Josiah Parsons Cooke (1827-1894) of Harvard. He graduated from college in 1848, and in 1850 became Erving Professor of Mineralogy and Chemistry, a position which he held for the remainder of his life.

Cooke's equipment for the duties of his new place was almost entirely the result of his own exertions. A course of lectures by the elder Silliman first aroused his enthusiasm for chemistry, and led him in early boyhood to fit up a laboratory in his father's house, where he attacked the science by experiment with such good results that even when he came to college he had a working knowledge of

the subject. At Cambridge he continued these studies essentially alone, as the chemical teaching of the college during his four years of residence was confined to five or six rather disjointed and fragmentary lectures. Immediately after appointment to his professorship he supplemented these meager preparations by obtaining leave of absence for eight months, which were spent in Europe buying apparatus and material and attending lectures by Regnault and Dumas. These formed the only instruction in chemistry he had received which could even claim to be systematic; yet with this slender outfit, aided by barely a year and a half of experience as a teacher, in 1851, at the age of 24, he found himself confronted with problems which would have taxed the abilities of an old, experienced and fully educated professor. Chemical teaching in Harvard College had become extinct and must be re-established. The college was wedded to methods of teaching, excellent for classics and mathematics, but entirely unfit for a subject like chemistry. These must be replaced by better methods, many of which were still to be invented. Finally he was called upon to take a prominent share in the great battle to introduce science into the college course on an equality with the humanities.

The zeal with which he threw himself into these tasks led to substantial results much more quickly than could have been expected. After only seven years he had succeeded in introducing required courses of chemistry into the sophomore and junior years. These, however, were only lecture and text-book courses; so that really a much more important advance consisted in the fact that he had also induced the faculty of the college to accept an elective

Josiah Parsons Cooke

course in qualitative analysis, to be taught in the laboratory by the experimental method. It is noteworthy that from the very beginning of his career, Cooke was an ardent adherent of the laboratory method of teaching chemistry invented not many years earlier by Liebig. This seems at first sight a strange breadth of view in a self-taught chemist, but, as he was fond of saying, the fact that he had taught himself chemistry by his own experiments showed him the value of this method for other students. But this was not all; a large building, Boylston Hall, had been built for the use of chemistry and comparative anatomy with money a large part of which had been raised by his exertions.

After this brilliant beginning the progress was continuous, until at the time of his death there were sixteen courses in chemistry and mineralogy, chosen by three hundred and fifteen students, and taught by three professors, three instructors and eight assistants. Boylston Hall was devoted exclusively to chemistry, and the Mineralogical Department was established in a section of the University Museum, also built through his exertions, where was exhibited the rich mineralogical collection created by him.

Cooke's scientific activity began even during his first struggles for the recognition of chemistry by the college (1854). In that year he published his first large experimental research, "On the Alloys of Zinc and Antimony." Some shorter papers on spectroscopic, crystallographic, and analytical subjects appeared. In 1866 he made the striking discovery of "danalite" and other new minerals in Rockport. He then made an extended research, both analytical and crystallographic, on the vermiculites and chlorites.

In 1873 he published the first of his most important series of researches, those on atomic weights, beginning with the vexed question in regard to the atomic weight of antimony. The result was a series of papers in which he established the atomic weight of that metal to the whole satisfaction of the entire chemical world. In connection with this work he made a careful study of some of the compounds of antimony with the halogens, in which, by the use of crystallographic methods, he succeeded in giving a probable explanation of the dimorphism of antimonious iodide.

The last of these researches was a careful redetermination of the relation between the atomic weights of oxygen and hydrogen. The experimental difficulties were even greater than those encountered in his work with antimony, but one by one they were overcome, and he was able to publish sixteen successive determinations showing a wonderfully close agreement, but, as Lord Rayleigh almost immediately pointed out, these results contained one of his old enemies—a constant error—due to the contraction of the glass globes when exhausted in order to weigh them empty. Cooke's last paper contained an ingenious method for avoiding this error by determining the tare of the globes without exhausting them.

In addition to the forty-one papers on his researches, he published thirty-two on other subjects, generally relating to chemistry, and eight books, ranging from such widely used text-books as the "Chemical Philosophy" and "New Chemistry" to works on the relation of religion and science, and an interesting volume of essays.

He enriched our stock of lecture apparatus with many

excellent contrivances, notably his arrangement for the projection of spectra, his form of the lecture-table eudiometer, and the vertical lantern.

His character is summed up in the words of his colleague, Professor H. B. Hill. "As an investigator, Cooke was clear in thought, persevering amid difficulties, fertile in expedients, impatient of dogma, and to the end he retained the keen curiosity and enthusiasm of his earlier days."

His books were: 1857, Chemical Problems and Reactions; 1860, Elements of Chemical Physics; 1864, Religion and Chemistry, or Proofs of God's Plan in the Atmosphere and Its Elements; 1868, Principles of Chemical Philosophy; 1874, The New Chemistry; 1881, Scientific Culture and Other Essays; 1888, The Credentials of Science the Warrant of Faith; 1891, Laboratory Practice.

Papers on his original investigation are: 1852, Description of a Crystal of Rhombic Arsenic; 1852, Octahedral Crystals of Arsenic; 1854, The Relation between the Atomic Weights; 1854, On Two New Crystalline Compounds of Zinc and Antimony; 1854, On a New Filtering Apparatus; 1855, On the Law of Definite Proportions in the Compounds of Zinc and Antimony; 1860, Crystalline Form Not Necessarily an Indication of Definite Chemical Composition; 1861, On the Dimorphism of Arsenic, Antimony and Zinc; On the Spectroscope; On the Cleavage of Galena; 1863, An Improved Spectroscope; 1863, Crystallographic Examination of Childrenite; 1864, Crystallographic Examination of the Acid Tartrates of Caesia and Rubidia; 1865, On a Spectroscope with Many Prisms; 1865, On the Projection of the Spectra of the Metals; 1866, On the Aqueous Lines of the Solar Spectrum; 1866, Separation

of Iron and Alumina; 1866, Analysis of Danalite of Rockport; 1867, On Cryophyllite; 1867, On Certain Lecture Experiments; 1867, Crystallographic Examination of Some American Chlorites; 1867, A Method of Determining the Protoxide of Iron in Silicates not Soluble in the Ordinary Mineral Acids; 1869, Atomic Ratio; 1874, The Vermiculites; 1875, Melanosiderite; 1875, On Two Varieties of Vermiculites; 1876, On a New Mode of Manipulating Hydric Sulphide; 1876, On the Process of Reverse Filtering; 1877, Revision of the Atomic Weights of Antimony; 1877, Re-examination of Some of the Haloid Compounds of Antimony; 1879, The Atomic Weight of Antimony; 1880, On the Oxidation of Hydrochloric Acid Solutions of Antimony in the Atmosphere; 1881, On the Solubility of Chloride of Silver in Water; 1881, Additional Experiments on the Atomic Weight of Antimony; 1881, The Boiling Point of Iodide of Antimony and a New Form of Air Thermometer; 1883, A Simple Method for Correcting the Weight of a Body for the Buoyancy of the Atmosphere When the Volume is Unknown; 1883, Possible Variability of the Law of Definite Proportions; 1887, The Relative Values of the Atomic Weights of Oxygen and Hydrogen; 1888, Additional Note on the Relative Values of the Atomic Weights of Oxygen and Hydrogen; 1889, On a New Method of Determining Gas Densities.

[C. L. Jackson in *Biographical Memoirs,* 4, 175, of the National Academy of Sciences.]

In searching through the early literature pertaining to the classification of the elements, there was discovered the following contribution of Cooke, which may well claim attention. It marks an attempt to solve the problem

of relationship of elements and their dependent properties. It is, perhaps, the first effort in this direction, made in our country, and for that reason is worthy of reproduction. It is entitled "The Numerical Relation between the Atomic Weights, with some Thoughts on the Classification of the Chemical Elements."

Numerical relations between the atomic weights of the chemical elements have been very frequently noticed by chemists. One of the fullest of these relations was that given by M. Dumas of Paris, before the British Association for the Advancement of Science, at the meeting of 1851. This distinguished chemist at that time pointed out the fact, that many of the elements might be grouped in triads, in which the atomic weight of Bromine is the mean between those of Chlorine and Iodine; that of Selenium is the mean between those of Sulphur and Tellurium, and that of Sodium, the mean between those of Lithium and Potassium. M. Dumas also spoke of the remarkable analogies between the properties of the members of these triads, comparing them with similar analogies observed in Organic Chemistry, and drew, as is well known, from these facts arguments to support the hypothesis of the compound nature of many of the now received elements. Similar views to those of Dumas have been advanced by other chemists.

The doctrine of triads is, however, as I hope to be able to show in the present memoir, a partial view of this subject, since these triads are only parts of series similar in all respects to the series of homologues of Organic Chemistry, in which the differences between the atomic weights of the members is a multiple of some whole number. All the elements may be classified into six series, in each of which this number is different, and may be said to characterize its series. In the first it is nine, in the second eight, in the third six, in the fourth five, in the fifth four, and

in the last three. The discovery of this simple numerical relation, which includes all others that have ever been noticed was the result of a classification of the chemical elements made for the purpose of exhibiting their analogies in the lecture-room. A short notice of this classification will, therefore, make a natural introduction to the subject.

Every teacher of Chemistry must have felt the want of some system of classification like those which so greatly facilitate the acquisition of the natural-history sciences. In most elementary text-books on Chemistry, the elements are grouped together with little regard to their analogies. Oxygen, Hydrogen, and Nitrogen are usually placed first, and therefore together, although there are hardly to be found three elements more dissimilar: again, Phosphorus and Sulphur, which are not chemically allied, are frequently placed consecutively, while Arsenic, Antimony, and Bismuth, in spite of their close analogies with Phosphorus, are described in a different part of the book. This confusion, which arises in part from retaining the artificial classification of the elements into metals and metalloids, is a source of great difficulty to the learner, since it obliges him to retain in his memory a large number of apparently disconnected facts. In order to meet this difficulty, a classification of the elements into six groups, differing but slightly from that given in the table accompanying this memoir, was made. The object of the classification was simply to facilitate the acquisition of Chemistry, by bringing together such elements as were allied in their chemical relations considered collectively. As the classification has been in use for some time in the courses of lectures on Chemistry given in Harvard University, I have had an opportunity for observing its value in teaching, and cannot but feel that the object for which it was made has been in a great measure attained. The series which is headed The Six Series will illustrate the advantage gained from the classification

in the course of lectures, the elements which compose it being among those specially dwelt upon in lectures to medical students, and, generally, very widely separated in a text-book on the science. As Chemistry is usually taught, the properties of the members of this series, Nitrogen, Phosphorus, Arsenic, and Antimony, as well as the composition and properties of their compounds, make up a large body of isolated facts, which, though without any assistance for his memory, the student is expected to retain. Certainly it cannot be wondered at, that he finds this a difficult task. The difficulty can, however, be in a great measure removed, if, after he has been taught that Nitrogen forms two important acids with Oxygen, NO_3 and NO_5, that it unites with Sulphur and Chlorine to form NS_3 and NCl_3, and also with three equivalents of Hydrogen to form NH_3, he is also told, that, if in these symbols of the Nitrogen compounds he replaces N by P, As/ or Sb, he will obtain symbols of similar compounds of Phosphorus, Arsenic, and Antimony; for he thus learns, once for all, the mode of combination of all four elements, so that when he comes to study the properties, in turn, of Phosphorus, Arsenic, and Antimony, he has not to learn with each an entirely new set of facts, but finds the same repeated with only a few variations. Moreover, these very variations he will learn to predict, if he is shown that the elements are arranged in the series according to the strength of their electro-negative properties, or, in other words, that their affinities for Oxygen, Chlorine, Sulphur, etc., increase, while those for Hydrogen decrease, as we descend. He will then readily see why it is that, though Nitrogen forms NO_3 and NO_5, it forms only NCl_3 and NS_3, and that this reason is correct he will be pleased to find confirmed when he learns that Phosphorus, which is more electro-positive than Nitrogen, and has, therefore, a stronger affinity both for Chlorine and Sulphur, forms not only PCl_3 and PS_3, but also PCl_5 and PS_5. Again, he will not be surprised, after

seeing the affinity of the elements for Hydrogen, growing constantly weaker as he descends in the series, to learn that a compound of Bismuth and Hydrogen is not certainly known. Should he inquire why, though NH_3 has basic properties, PH_3, AsH_3 and SbH_3, have not, he can be shown that the loss of basic properties in passing from NH_3 to PH_3 corresponds to a decrease in the strength of the affinity between the elements, and that if in PH_3, SbH_3, or AsH_3, atoms of Methyle, Ethyle, or other organic radicals analogous to Hydrogen, are substituted for the Hydrogen atoms, and more stable compounds thus obtained, strong bases are the result. The other series would afford similar illustrations, and, from my own experience, I am confident that no teacher who will once use the classification of the elements here proposed, or one similar to it, will ever think of attempting to teach Chemistry without its aid.

Classifications of the elements, more or less complete, have been given by many authors; but the fact that no one has been generally received, is sufficient to prove that they are all liable to objections, and would, indeed, also seem to show that a strictly scientific classification is hardly possible in the present state of the science. The difficulty with most of the classifications is, undoubtedly, that they are too one-sided, based upon one set of properties to the exclusion of others, and often on seeming, rather than real resemblances. This is the difficulty with the old classification into metals and metalloids, which separated Phosphorus and Arsenic, Sulphur and Selenium, because Arsenic and Selenium have a metallic lustre, while Phosphorus and Sulphur have not, though there could hardly be found another point of difference. For a zoologist to separate the ostrich from the class of birds because it cannot fly, would not be more absurd, than it is for a chemist to separate two essentially allied elements, because one has a metallic lustre and the other has not. Yet it is surprising to see how persistently this classification is retained

in every elementary work on the science; and if it is sometimes so far modified as to transfer elements analogous to Selenium and Arsenic to the class of metalloids, this is only acknowledging the worthlessness of the principle, without being willing to abandon it. If there were any fundamental property common to all the elements, the law of whose variation was known, this might serve as the basis of a correct classification. Chemistry, however, does not as yet present us with such a property, and we must, therefore, here, as in other sciences, base our classification on general analogies. The most fundamental of all chemical properties is, undoubtedly, crystalline form, but a classification of the elements based solely on the principles of isomorphism is defective in the same way as it is in mineralogy. It brings together, undoubtedly, allied elements, but it also groups with them those which resemble each other only in their crystalline form. The mode of combining seems to be also a fundamental property; but like crystalline form it would bring together in some instances elements differing very widely in their chemical properties. A classification of the elements which shall exhibit their natural affinities, must obviously pay regard to both of these properties. It must at the same time seek to group together isomorphous elements, and those which form analogous compounds. Moreover, in such a classification, other less fundamental properties must not be disregarded. There are many properties both physical and chemical, which, although they cannot be exactly measured, and are oftentimes difficult to define (such properties as those by which a chemist recognizes a familiar substance, or a mineralogist a familiar mineral), and which on account of their indefinite character cannot be used as a basis of classification, may, nevertheless, render important aid in tracing out analogies. Judging from such properties as these, chemists are generally agreed in grouping together Carbon, Boron, and Silicon, although they cannot be proved

to be isomorphous, and are not generally thought to form similar compounds.

It is, however, much easier to point out what a classification should be, than to make one which shall fulfil the required conditions. Indeed, as has been already said, past experience would seem to show that a perfect scientific classification of the elements is hardly possible in the present state of Chemistry. At best, the task is attended with great difficulties, and it cannot be expected that these should be surmounted at once. The classification which is offered in this memoir will, undoubtedly, be found to contain many defects. If, however, it is but one step in advance of those which have preceded it, it will be of value to the science. It was originally made, as has already been said, simply for the purpose of teaching, and never would have been published had it not led to the discovery of the numerical relation between the atomic weights.

On turning to the table which accompanies this memoir, it will be seen that the elements have been grouped into six series. These correspond entirely to the series of homologues of Organic Chemistry. In the group of volatile acids homologues of Formic Acid, for example, we have a series of compounds yielding similar derivatives, and producing similar reactions, and many of whose properties, such as boiling and melting points, specific gravity, etc., vary as we descend in the series according to a determinate law. From Formic Acid, a highly limpid, volatile, and corrosive fluid, the acids become less and less volatile, less and less fluid, less and less corrosive; first oily, then fat-like, and finally hard, brittle solids, like wax. As is well known, the composition of these acids varies in the same way, and the variation follows a regular law, so that by means of a general symbol we can express the composition of the class. This symbol for the volatile acids may be written $(C_2H)\ O_3,\ HO+n\ (S_2H_2)$.

This description of the well-known series of the volatile

acids, applies, word for word, nominibus mutantis, to each of the six series of chemical elements. The elements of any one series form similar compounds and produce similar reactions; moreover, they resemble each other in another respect in which the members of the organic series do not. Their crystalline forms are the same, or, in other words, they are isomorphous. Although this may be true of the volatile acids, yet it cannot be proved in the present state of our knowledge. Still further, many of their properties vary in a regular manner as we descend in the series. In one case, at least, the law of the variation is known, and can be expressed algebraically, though in most instances it cannot be determined. Finally, as one general symbol will express the composition of a whole organic series, so a simple algebraic formula will express the atomic weight, or, if you may be pleased so to term it, the constitution of a series of elements.

These points may be illustrated with any of the series in the table; with the first, for example, which consists of Oxygen, Fluorine, Cyanogen, Chlorine, Bromine, and Iodine. All these elements form similar compounds, as will be seen by inspecting the symbols of their compounds given at the right hand of the list of names, where the similar or homologous compounds are arranged in upright columns. Moreover, they are all isomorphous, as may be seen by referring to the left hand side of the list, where the similar compounds in each upright series are isomorphous, the numbers at the heads of the columns indicating the systems of crystallization, as is described in the explanation accompanying the table. That the properties of these elements vary as we descend, can be easily shown. Oxygen is a permanent gas, as is also Fluorine. Cyanogen is a gas, but may be condensed to a liquid. Chlorine, a gas also, can be condensed more easily than Cyanogen. Bromine is a fluid at the ordinary temperature; and finally, Iodine is a solid. Moreover, starting from Cyanogen, the solubility of

these elements in water decreases as we descend in the series; and again, the specific gravity of their vapors follows the inverse order of progression, gradually increasing from Oxygen down. The atomic weights vary in the same order, and admit of a general expression, which is 8 in 9, or in other words, the differences between the atomic weights of these elements are always a multiple of nine. This general formula may be said to represent the constitution of these elements, in the same way that the symbol $(C_2H)\ O_3$, $HO+n\ (C_2H_2)$ represents the composition of the volatile acids before mentioned. In the place of $(C_2H)\ O_3$, HO we have $8=0=$ the weight of one atom of Oxygen and in the place of C_2H_2 we have nine. What it is that weighs nine (for it must be remembered that those numbers are weights) we cannot at present say, but it is not impossible that this will be hereafter discovered. In order to bring the general symbol of the volatile acids into exact comparison with that of the Nine Series, we must reduce the symbols to weights, when the two formulas become

$46+n\ 14$, where $46=(C_2H)\ O_3$, HO and $14=C_2H_2$;
and $8+n\ 9$, where $8=O$ and $9=x$.

The numbers 46 and 14 are known to represent the weights of aggregations of atoms. The number 8 represents the weight of one Oxygen atom, but we cannot as yet say what the 9 represents. After this comparison, it does not seem bold theorizing to suppose that the atoms of the members of this series are formed of an atom of Oxygen as a nucleus, to which have been added one or more groups of atoms, the weight of which equals nine, or perhaps one or more single atoms each weighing nine, to which the corresponding element has not yet been discovered. As it will be convenient to have names to denote the two terms of the formulas which represent the constitution of the different series, we will call the first term, in accordance with this theory, the

nucleus, and the number in the second term multiplied by n the common difference of the series.

From what has been said, it will be seen that the idea of the classification is that of the organic series. It is in this that the classification differs from those which have preceded it. Other authors in grouping together the elements according to the principles of isomorphism, have obtained groups very similar to those here presented. Indeed, this could not be otherwise, since, as has been already said, the members of each series are isomorphous, while, as a general rule, to which, however, there are many exceptions, no isomorphism can be established between members of different series. These groups, however, have been merely groups of isomorphous elements, and not series of homologous like those in which the elements are here classed.

These general remarks will suffice to indicate the principles upon which the classification has been made, and the character of the numerical relation between the atomic weights which has been established. The details of the classification can be best studied by referring to the table so that it will be only necessary to speak of those points which are of special interest, or which may require explanation, or in regard to which there may be doubt. The series I have named from their common differences. The first I have called the Nine Series, the second the Eight Series, &c. Let us examine the doubtful points in each, commencing with the first.

The last five members of the Eight Series are connected by so many analogies, that they have been invariably grouped together in the elementary books. There can be no doubt, therefore, in regard to the propriety of placing them in the same series, on the ground of general analogies. Fluorine, it is true, presents some striking points of difference from the rest. Fluoride of Calcium is almost insoluble in water, while the Chloride, Bromide, and Iodide of Calcium are all very soluble. We must, however, remember

that we have to do with series, and must not therefore expect to find close resemblances except between adjacent members. If, then, we consider that Oxygen is one of the series, and that Fluorine stands but one step removed from Oxygen, while it is two steps removed from Chlorine, the discrepancy in a measure vanishes, for Lime CaO is but slightly soluble in water. Nevertheless, the difficulty does not entirely disappear, for CaF is much less soluble than CaO, although it should be more soluble than CaF.

The solubility of a series of homologous elements or compounds in water, may be regarded as a function of one or more variables. In the case of elements there may be but one variable; but it is easy to see that in the case of compounds there must be several. One of these variables is probably the same which determines the common difference of the series to which the elements or compounds belong (it will be hereafter shown that the atomic weights of the homologous compounds are related in the same way as those of the elements); the other variables are perhaps the atomic forces which determine the hardness, density, &c., of the solid. We may, therefore, with justice, compare the relative solubilities of a series of homologues to a curve which should be the same function of the same variables, and what mathematics teaches we ought reasonably to expect in the case of this curve, we ought to expect also in the variations of solubility of these substances. Now every mathematician is familiar with the remarkably rapid changes which a curve undergoes that is a function of several variables, and we cannot be surprised that similarly rapid changes should be observed in the solubility of homologous substances in passing from one to the next in the series. In the curve which corresponds to the relative solubility of CaO, CaF, CaCy, CaCl, CaBr, and CaI, it would seem that at CaF there is a singular point where the curve, after rising for some distance above the axis, bends down again towards it. Several of the other series of com-

pounds of these elements present similar anomalies; for example, KO, KF, KCy, KCl, KBr, and KI. Here the solubility diminishes until we come to KCl, which is less soluble than KCy; then it increases to the last. Here, of course, the singular point is at KCl. With the corresponding compounds of Sodium, the solubility diminishes to NaF, which is the least soluble of the series, and then increases constantly to the end.

These facts at least seem to show that apparent variations from the law of series in properties, which evidently are unknown functions of several variables, should not be allowed to outweigh strong analogies, and certainly the analogies between Fluorine and the other haloids are very marked. Fluorine itself possessed properties such as we should expect to find in a member of the series above Chlorine. The strong and active affinities of Fluorine might be indeed predicted, after seeing the rapid increase both in the strength and activity of the affinities in passing from Iodine to Chlorine. In passing from Bromine to Chlorine, we pass from a liquid to a gas, permanent under any natural conditions; and we should expect, therefore, in rising still higher in the series, to find in Fluorine a gas less easily reduced to a liquid than Chlorine. Now although, on account of its remarkably active affinities, this fact cannot be demonstrated on the gas itself; it can, nevertheless, be inferred with perfect certainty from its compounds. Finally, the isomorphism of Fluorine, and the other haloids may be urged as indicating close analogy. From these considerations, I cannot but think that those chemists who have questioned the propriety of classing Fluorine with the other haloids will, on reviewing the facts, and regarding the haloids in the light of a series, and not simply as a group of elements possessing certain properties, be led to change their opinion.

Cyanogen, though a compound radical, has been classed with the other haloids, not only from its atomic weight,

but also from its other analogies. Its properties are in most cases those which we should expect from an element occupying its position in the series; but in others it presents remarkable variations, owing probably to the fact that it contains a radical which is easily decomposed. As well known, it is perfectly isomorphous with Chlorine.

The property of classing Oxygen in this series seems to be placed beyond doubt by the discovery of Ozone, which, though it does not seem to possess such energy as we should expect in an element higher in the series than Fluorine, may, nevertheless, be found to fulfil all anticipations should it ever be obtained in a perfectly unmixed condition. The isomorphism of Oxygen with Chlorine, and therefore with the other haloids, seemed sufficiently established by the determination both of Proust and Mitscherlich of the tetrahedral form of Cu_2 Cl. It must, however, be admitted that Oxygen presents as strong analogies with Sulphur as it does with Chlorine; and since, not only from its analogies, but also from its atomic weight, it appears to be the nucleus on all the first three series, I have placed it at the head of each. It may be mentioned here, that in all cases the fact that the atomic weight of an element is included in the general formula of a series, is an argument for classifying it in that series, if the relation between the atomic weights pointed out in this memoir is admitted to be a law of nature; but as I wish to show that the relation is not that of a mere accidental group of numbers, but is connected with the most fundamental properties of the elements, and has, therefore, the claims of a law, I have endeavored to establish the correctness of the classification which conforms to the law, and, indeed, suggested the law on other grounds.

The atomic weights of the members of the Nine Series, as determined by experiment, present greater deviations from the numerical law already explained, than are to be found in any of the others. The weights which would exactly

conform to the general formula 8 + n9 are given in the column of the table headed Theoretical, while in the next column at the right are given the weights of experiment. These for the most part (in this as well as in the other series) have been taken from the table of Atomic Weights given in the last volume of Liebig and Kopp's *Jahresbericht* (for 1852), which was supposed to give the most accurate and latest results. In the few cases in which the numbers have not been taken from this table, the initial letter of the name of the observer has been annexed. It will be seen, on comparing the two columns that the greatest deviation from the law is in the case of Fluorine, if we consider the care which was taken both by Berzelius and Louyet, in the determination of the atomic weight of this element. It may, however, be remarked, that, as the processes used by both experimenters were essentially identical, any hidden constant source of error would produce the same effect on both results; so that the atomic weight of Fluorine cannot be regarded as yet as absolutely fixed. Nevertheless, it is not possible that so great a difference between the true and observed weights as two units could have escaped detection in the numberless analyses which have been made, by the most experienced chemists, of the Fluorine compounds. It must, therefore, be admitted, and not only in the case of Fluorine, but also in other instances, that there are deviations from the law; but these deviations are not greater than those from similar numerical laws in astronomy and other sciences, and indeed, judging from the analogy of these sciences, ought to be expected.

Those who are not familiar with the amounts of probable error in the determination of the different atomic weights would judge, on comparing together the columns of theoretical and observed values, that the deviations from the law were much greater than they are in reality. It should, therefore, be stated, that, in by far the larger number of instances, the deviations are within the limit of possible

errors in the determinations, leaving only a few exceptional cases to be accounted for. It must be remembered that, other things being equal, the amount of probable error is greater the greater the atomic weight, so that a difference of 1.9 in the case of Iodine is not a greater actual deviation from the law than only 0.5 in the case of Chlorine. Indeed, it is very possible that on more accurate determinations the atomic weight of Iodine will be found to correspond to the law, which cannot be expected of that of Chlorine. It is well known that many of the larger atomic weights, especially those of the rarer elements, cannot be regarded as fixed within several units.

I have calculated, as well as the data I have would permit, the amount of probable error in the determinations of many of the atomic weights, and by comparing together the results from different processes, and by different experimenters, I have endeavored to detect the existence of constant errors, which seem to be the great errors in all these determinations, those accidental errors which are made in the repetitions of the same process by equally careful experimenters being comparatively insignificant. The results of this investigation will be published in a subsequent memoir. It is sufficient for the present purpose to state, that, while they show that, in the greater number of cases, the apparent variations from the law are within the limit of probable error, there are yet several instances, where, after allowing for all possible errors of observation, there is a residual difference. I do not therefore look alone to more accurate observations for a confirmation of the law, but regarding the variations as ascertained facts, hope that future discovery will reveal the cause. Whether the variations will be found to be a secondary result of the very cause which has determined the distribution of the atomic weights according to a numerical law, as the perturbations in astronomy are a necessary consequence of the very law they seemed at first to invalidate, or whether they

are due to independent causes, can of course, for the present, be only a matter of speculation. There are, however, facts which seem to indicate that the variations are not matters of chance, but correspond to variations in the properties of the elements.

From the beautiful discovery of Professor Schönbein we have learnt that Oxygen has two allotropic modifications, and that besides its ordinary condition it is capable of assuming another highly active state when its properties resemble those of Chlorine. Cyanogen is only known in a quiescent state. The other haloids, Fluorine, Chlorine, Bromine, and Iodine are only known in a highly active state. Now it will be seen on examining the table that the atomic weights of the highly active elements, as determined by experiment, exceed slightly the theoretical numbers, and that where the affinities are the most intense, in Fluorine, the deviation is the greatest. A similar fact may be observed in the atomic weights of the members of the Six Series. Arsenic has been proved to be capable of existing in two allotropic modifications. In its ordinary state, it has a crystalline form belonging to the Rhombic System. In the other condition, in which it may be obtained by sublimation at a low temperature, it crystallizes in regular octahedrons. The other members of this series are probably isodimorphs with Arsenic. The ordinary condition of Phosphorus is its monometric modification, while the rhombic state seems to be the normal condition of Arsenic, Antimony, and Bismuth. Now the atomic weights of the last three are either equal to, or slightly exceed, the theoretical number, while that of the first fall short, perhaps even by a unit. Other facts, which also tend to show that the deviations are not matters of chance, may be found in the affiliations of the series. There are some elements which seem to be most remarkably double-faced, having certain properties which connect them closely with one series, and at the same time others which unite them nearly as closely to an-

other. In such cases we find that the atomic weight either falls naturally into both series, or, not corresponding exactly with the theoretical number of the series to which the element properly belongs, it inclines towards that of the other, and sometimes equals it. Such is the case with Chromium, Manganese, and Gold, as will be seen by referring to the affiliations at the bottom of the Nine Series. These various facts force upon me the conviction, that this relation between the atomic weights is not a matter of chance, but that it was a part of the grand plan of the Framer of the universe, and that in the very deviations, from the law, there will, hereafter, be found fresh evidence of the wisdom and forethought of its Divine Author.

The general formulæ for the Eight Series are 8+n8 and 4+n8. The two nuclei correspond to two different sets of elements, or sub-series, one consisting of Oxygen, Sulphur, Selenium, and Tellurium, the other of Molybdenum, Vanadium, Tungsten, and Tantalum. The atomic weights of the first are all equal to 8+n8; those of the second to 4+n8. The sub-series exhibit marked analogies, as well as certain differences. They resemble each other chiefly in that the members of both form analogous acids with Oxygen, while they differ in that, though the members of the first sub-series form compounds with Hydrogen, those of the second do not. The isomorphism of the members of each sub-series among themselves, with the exception of Vanadium, is complete; but there seems to be no proof of any isomorphism between the sub-series. Johnston attempted to establish the isomorphism of Chromic and Molybdic Acids from the red variety of Molybdate of Lead from Rezbanya, which he supposed to be a Chromate; but the fact has been disproved by G. Rose, who has shown that the supposed Chromate is a Molybdate mixed with a small amount only of Chromate. There seems, nevertheless, to be some reason for believing that Chromic Acid may replace Molybdic Acid to a certain extent. If this is proved, it establishes

another link of connection between the members of the two sub-series, since Chromic Acid is isomorphous with Sulphuric Acid. For the present, however, we must regard them as sub-series, related, but distinct, the second being in a measure supplementary to the first. They are distinguished in the table by printing the names of the second sub-series a little to the right of those of the first, and the fact that their atomic weights are intermediate to those of the first, I have indicated to the eye by giving to the names also an intermediate position.

The analogies between Oxygen and Sulphur are so numerous, that were we to place Oxygen in but one series, we should place it in this. HO and HS, HO_2 and HS_2, resemble each other very closely, as do also the Oxygen salts the corresponding Sulphur salts. Moreover, there can be no doubt in regard to the isomorphism of the two elements, since it has been established upon the authority both of Mitscherlich and Becquerel, and from two different compounds. The only doubtful case in the series was that of Vanadium, which in some of its properties resembles Arsenic more closely than it does Molybdenum. The reasons for giving it the place which it occupies were the facts that its acids correspond to those of Molybdenum, and that it forms remarkably highly colored oxides which are repeated also in Molybdenum. It is true that the properties of the element itself are not those we should expect from the position which it occupies in our table; yet, if it were placed in the Six Series, it would fall between Phosphorus and Arsenic, which on the whole it resembles less than it does Molybdenum, for although it is combustible, yet neither it nor its oxides are volatile. I consider it, therefore, as a member of the Eight Series, but affiliating very closely with the Six. Its atomic weight favors this hypothesis. Vanadate of Lead has been considered isomorphous with the Phosphate; but as this isomorphism does not rest on any measurement of angles, and as, moreover, the re-

ceived symbols of the two minerals, Vanadinite and Pyromorphite, on whose crystalline forms the isomorphism was determined, show a very different constitution, I have not given much weight to this fact.* The observed atomic weights of the members of this series are almost precisely the same as theoretical members, and, with the exception, perhaps, of Molybdenum, there appears to be no instance in which the difference is greater than the amount of possible error.

The members of the Six Group form a well-characterized family, so that, with the exception of Oxygen, there can be no doubt in regard to the justice of classifying them together, and any discrepancies will disappear on considering the group in the light of a series. They form acids containing three and five atoms of Oxygen which are completely homologous, and make two series parallel to that of the elements. They form also a remarkable series of compounds with three atoms of Hydrogen. The idea which has been advanced by some authors, that NH_3 is the Nitride of Hydrogen, while PH_3 is the Hyduret of Phosphorus, or, in other words, that Hydrogen is electro-positive with reference to Nitrogen and electro-negative with reference to Phosphorus and those lower in the series, does not seem to me correct, since the remarkable bases which may be formed from PH_3, AsH_3, SbH_3, and BiH_3, by replacing the Hydrogen atoms by organic radicals, seem to indicate that they have the same type as NH_3, and are therefore homologues of it.

The isomorphism of the four lower members of the series is perfect. It has been shown in the table, both by the crystalline forms of the elements themselves, as well as by those of their compounds. In the other series, wherever it was possible, the same double proof has been given. The doubt expressed by G. Rose in regard to the dimorphism of Arsenic, as I hope to be able to show in a paper

* See G. Rose's Mineral System.

soon to be published, has been removed. In one state Arsenic crystallizes in perfect octahedrons of the regular system, and is therefore isomorphous, not only with Antimony and Bismuth, but also, in its allotropic state, with Phosphorus. Isomorphism, as is well known, is not absolute, except in forms of the regular system. The rhombic angles of the crystals of Arsenic, Antimony, and Bismuth are respectively 85° 41′, 87° 35′, 87° 40′, and therefore conform to the general rule. It will be observed that the angle varies constantly in the same way as we descend in the series. Now, although these few instances do not afford sufficient ground for any general conclusion, yet they show that similar variations are possible in the other systems, and therefore that we cannot be expected to establish isomorphism in any case except between nearly consecutive members.

The atomic weights of the members of this series, with the exception of Phosphorus, do not present any important deviations from the theoretical numbers, taking into account always, of course, the amount of possible error. The deviation in the case of Phosphorus has already been noticed. Oxygen, it must be admitted, is not connected with the series from any similarity of properties, though the Phosphides, Arsenides, and Antimonides present certain analogies with the Oxides. As has already been said, Oxygen was placed at the head of this, as well as of the last two series, its atomic weight seemed to be the nucleus of all three.

The Five Series is the shortest of all, consisting of only three members, Carbon, Boron, and Silicon. Of these, the last two are as closely allied as are any two members of the other series, Silicon having precisely the properties we should expect in a homologue of Boron, which was lower in the series; and the same is also true of their compounds. The analogies, however, between these two elements and Carbon are by no means so close, for not only Carbon can-

not be proved to be isomorphous with them, but it does not form similar compounds. Carbonic Acid, it is true, presents some points of resemblance to Boracic and Silicic Acids; like them it unites in a large variety of proportions with bases, its alkaline salts give a basic reaction, &c.; but according to the generally received opinion, its symbol is CO_2, while those of Boron and Silicon are BO_3 and SiO_3. In its uncombined state, however, Carbon resembles Boron and Silicon, not only in its outward properties, but also in its action before the blowpipe. Two of the allotropic states of Carbon, Graphite, and Charcoal, are probably repeated in Boron, and are known to be in Silicon. The principle of exclusion would seem to place Carbon in this series, for it certainly presents no analogies with the members of any other. The correspondence of the atomic weights of the members of this series to the law is remarkably close.

The Four Series is by far the largest of all, including the greater number of what are generally known as the heavy metals. The members of the series resemble each other in the following respects, First, they are isomorphous; for though each member cannot be directly proved to be isomorphous with every other, yet isomorphism can be established between consecutive members, which, as has before been said, is all that can be expected. Second, the members of this series all form, by uniting with Oxygen, either Protoxides or Sesquioxides, or both, which, as a general rule, are strong bases. Third, these Oxides are either insoluble, or nearly insoluble, in water. And finally, the elements of the series have all those physical properties which are known as metallic properties.

This series may be naturally divided into two sub-series. The first contains those elements whose protoxide bases are their characteristic compounds, and which do not form acids with Oxygen. The second contains those elements whose characteristic compounds are their sesquibases. They generally unite with two or more equivalents of Oxygen,

and form acids. These sub-series are distinguished in the table in the same way as those of the Six Series. Corresponding to these sub-series we have two sets of atomic weights, each having the same common difference, but differing in their starting point or nucleus. The first set expressed by the formula 4+n4, the second by 2+n4.

The sub-series affiliate with each other in a most remarkable manner. Manganese, for example, not only forms a strong protoxide base, but also unites with a larger amount of Oxygen, forming both a sesquibase and acids. Its atomic weight places it in the first group, and it has therefore been classed there, although by its properties it is equally allied to the second. Cobalt and Nickel certainly resemble much more closely the members of the first than of the second sub-series, although their atomic weights place them in the second. With this exception, the subdivision of the series which the atomic weights require does not differ from that suggested by the properties of the elements. The members of this series may of course be still further subdivided into groups according to their special properties, as they are in all works on Chemistry. They are placed together here because the atomic weights form but one numerical series.

The isomorphism of the members of this series will be found well established with the limitations before given. In order to establish the isomorphism of Cobalt and Nickel with Iron, the isomorphism of one atom of Arsenic with two atoms of Sulphur has been assumed. This is generally admitted; but if it is not, no one can doubt in regard to the isomorphism of these three metals, as they constantly replace each other. Glucinum, Zirconium, Lanthanum, Cerium, and Thorium cannot be shown to be isomorphous with the other metals by any of their compounds, but their oxides are known to replace the analogous oxides of the other metals. So also is Ruthenium known to replace Rhodium. There have been doubts expressed in regard to the

existence of a monometric form of Zinc; but as we have established its isomorphism with the other members of the series, not only by its own crystalline form, but also by those of its compounds, the fact is of no importance to the present question. The atomic weights of the members, as determined by observation, very nearly correspond with the theoretical numbers, which is the more remarkable, as the limit of error in the determination of the atomic weights of the greater number, especially of the rarer metals, is quite wide.

The Three (and last) Series is composed of Hydrogen and the metals of the alkalies. The analogies between Lithium, Sodium, and Potassium are very close, as is well known, and there can be no doubt in regard to the propriety of classing them together. It may be said, however, in regard to Hydrogen, that it resembles as closely some of the metals of the Four Series as it does those of the alkalies. Though this cannot be denied, yet the fact that the atomic weight of Hydrogen is the nucleus of the series, and the great solubility of the alkalies in water, may be urged as reasons for placing it at the head of the Three Series.

The isomorphism of Lithium, Sodium, and Potassium is fully established; but I can find no data which prove Hydrogen isomorphous either with them or with the metals of the other group.

The unit of the atomic weights which has been used thus far throughout the table, is the double atom of Hydrogen; but the nucleus of the Three Series is the weight of the single atom, so that the unit in this series is one half of the unit of the weights in all the other series. This fact must be kept in mind in comparing the atomic weights of this with those of the other series. All the weights might have been made uniform by doubling them throughout; but as this would not have changed the relation, and would have been departing from the general custom, it was

thought best to confine the doubling to the Three Series, into which alone Hydrogen enters. The general symbol of this series is 1+n3, where of course the unit is one half of that of the symbols at the head of the other series. The observed atomic weights will be found to correspond very closely with the theoretical numbers; indeed, the two coincide, except in the case of Potassium, where the difference is 0.6. This, however, it must be remembered, is 0.6 of the single Hydrogen atom. Compared with the double atom, as the weight of Potassium is generally given, the difference amounts to but 0.3.

One of the most remarkable points of the classification which has been now explained, is the affiliation of the series. We find in Chemistry, as in other sciences, that Nature seems to abhor abrupt transitions, and shades off her bounding lines. Many of the elements, while they manifestly belong to one series, have properties which ally them to another. Several examples of this have already been noticed. In such cases, we find invariably that there is a similar affiliation of the atomic weight. Of all the elements Chromium and Manganese are the most protean. Two atoms of these elements unite with seven atoms of Oxygen and form acids analogous to Perchloric Acid, and, as has already been shown, the weight of two atoms of either element falls into the Nine Series. Moreover, one atom of Chromium or of Manganese unites with three atoms of Oxygen to form Chromic or Manganic Acid. Chromic Acid is a strong oxidizing agent, and resembles closely Nitrous Acid, and the atomic weight of Chromium falls into the Six Series just below that of Nitrogen. Manganic Acid, on the other hand, resembles Sulphuric Acid, with which it is isomorphous, and the atomic weight of Manganese would place it in the Eight Series. In like manner Osmium in many of its properties resembles Platinum and the other metals with which it is associated in nature; but, unlike them, it forms a very remarkable volatile acid, whose in-

supportable and suffocating odor as well as composition reminds one of the acids of the Nine Series, and its atomic weight seems to justify the apparent analogy. Gold likewise, though the noblest of metals, yet in some of its chemical relations resembles much more closely the members of the Nine than of the Four Series, and here again its accommodating atomic weight seems to account for its double-sided character. Several other examples of similar affiliations are given in the Table, but do not need explanation.

In the description just concluded of the classification of the chemical elements, which is offered in this memoir, I have not entered into details, for to have done so would have been to write a treatise on Chemistry. I have confined myself almost exclusively to general points, and only referred to those particulars which I thought might present doubts. I hope that I have been able to show, first, that the chemical elements may be classified in a few series similar to the series of homologues of Organic Chemistry; second, that in those series the properties of the elements follow a law of progression; and finally, that the atomic weights vary according to a similar law, which may be expressed by a simple algebraic formula. As already intimated, I have endeavored to prove the correctness of the classification on general grounds, in order that it might appear that the simple numerical relation which has been discovered between the atomic weights is not a matter of chance, but is connected with the most fundamental properties of the elements. I might leave the subject at this point, but the existence of the law which I wish to establish will be proved more conclusively if it can be shown, not simply that the general properties of the members of each series vary in a regular manner, but also if in one or more cases the exact law of the variation can be pointed out.

There are but few properties of the elements which are subjects of measurements, and which therefore can be

compared numerically. Such are the specific gravity in the three states of aggregation, the boiling and melting points, the capacity of heat, and a few others. It is easy to see that there are but few of these properties the law of whose variation in the series we could reasonably expect to discover in the present state of existence. Most of them evidently depend upon molecular forces with which we are entirely unacquainted. Such in solids is undoubtedly the case with so simple and fundamental property as specific gravity, and most, if not all, of the other properties of solids belong to the same category. It cannot therefore be expected that we should point out the laws by which these properties vary, although the remarkable investigations of Dana, Filhol, Kopp, Schröder, and others, on the relations between the density of substances and their atomic weights, and those of Kenngott on the relation of hardness to atomic volume, give grounds for expecting that even they will before long be discovered. In liquids and gases, however, most of these molecular forces which produce the apparent irregularities in solids have less influence, as we should naturally expect, probably because the atoms are removed out of the sphere of their action. We may therefore hope, on comparing together the properties of the liquid or gaseous status of the elements in any series, to discover some numerical relation between them. Unfortunately, however, we have not sufficient data for making such a comparison except in the case of one property, the specific gravity. The boiling point, which would be a very valuable property for the purpose, is known only in a few instances.

That the specific gravity of the elements in their gaseous state varies in each series according to a numerical law, follows necessarily from what is already known. It is a well-known fact, that the specific gravities of the gaseous states of the elements divided by their atomic weights give quotients which are either equal, or which stand in a very

simple relation to each other. For any series, as far as we have data, this quotient is the same for all the elements with only a few exceptions. That is $\frac{\text{Sp. Gr.}}{\text{At. W.}} = \text{p}$. But we have found that At. W. may be expressed in general by a+nb, and substituting this for At. W. in the above equation, it becomes $\frac{\text{Sp. Gr.}}{\text{a+nb}} = \text{p}$, or Sp. Gr.$=$p a$+$n p b; so that p a$+$n p b is a general expression for the specific gravity of all the elements of any series, in the same way that a$+$n b is for the atomic weight. The value of p will differ according as the specific gravities used are referred to Hydrogen or Air. Below will be found tables which give the calculated and observed specific gravities of the elements of the Nine and Six Series referred to Hydrogen, which has been taken as the unit instead of Air, as we thus in great measure avoid fractions. In the Nine Series p$=$1 so that the numbers representing the specific gravities are the same as those representing the atomic weights. In the Six Series it equals two, so that the numbers representing the specific gravities are in this series twice as large as those representing the atomic weights. When the specific gravity has not been observed, the calculated number only is given. The observed numbers are taken from the "Table of Specific Gravity of Gases and Vapors," in Graham's *Elements of Chemistry,* which is a very complete collection of all known data. For the other series, we have only occasional data, so that it is not possible to give complete tables of their specific gravities.

It is evident, then, that at least one property of the elements varies in the series according to an ascertained numerical law. But, it may be said, this proves nothing, for these specific gravities are connected so closely with the atomic weights that what is true of the one must be to the same extent true of the other. It must be remembered, however, that the specific gravities are a distinct set of

The Nine Series $\frac{\text{Sp. Gr.}}{\text{At. W.}} = 1$ Sp. Gr. = 8 + n9			The Six Series $\frac{\text{Sp. Gr.}}{\text{At. W.}} = 2$ Sp. Gr. = 16 + n12		
Names	Specific Gravities		Names	Specific Gravities	
	Theoret.	Observed		Theoret.	Observed
Oxygen	8	16	Oxygen	16	16
Fluorine	17	..	Nitrogen	28	14
Cyanogen	26	26	Phosphorus	64	64
Chlorine	35	35.5	Arsenic	148	150
Bromine	80	78	Antimony	256	...
Iodine	125	126	Bismuth	412	...

observed facts, and that the probability of a law is in exact proportion to the number of facts which accord with it. Moreover, the closeness of the connection is unimportant. Whether the value of p be expressed by a single digit, or by a complicated algebraic formula, is evidently a matter of indifference so far as the confirmation of the law is concerned.

In this memoir I have confined myself entirely to the elements, but it is evident that the classification here offered, and the numerical laws here explained, may be extended to all compounds. The elements of any one series, by combining, give rise to perfectly parallel series of homologous binaries, some of which are given in the table. The binaries of those series which have the greatest common difference are generally acids; and of those which have the smallest, they are generally bases. These acids and bases unite together and form series of homologous salts. As in Organic Chemistry, many of the series are very incomplete; but they are much more generally perfect than

in that newer department of the science, and almost every day fills up some gap.

It will be seen, then, that not merely a plan has been given for classifying the elements, but one which will also embrace all inorganic compounds, and affiliate with the similar classification which has already been established in Organic Chemistry. We have not attempted to develop such a classification, since to do it would require a volume, nor is it necessary, as any one can develop it for himself.

That the atomic weights of the series of homologous compounds follow the same numerical law as those of the elements is easily shown. Take as an example the series of salts homologous with KO, NO_5, which may be expressed in general by KO, RO_5, where R is any member of the Six Series after Oxygen, and whose atomic weight, therefore, equals 8+n6. The atomic weight of KO, RO_5, must be necessarily 39.5+48+(8+n6), or 95.5+n6. As this symbol differs from that of the Six Series only in the nucleus, the atomic weights of the salts which are represented by it must progress by the same differences as those of the corresponding elements.

The properties of these series of homologous compounds will also be found to vary in a regular manner, and the law of the progression of the specific gravities in the gaseous state can be easily expressed algebraically, since in each series the quotient of the specific gravity divided by the atomic weight is a constant quantity. As an illustration, we may take the series of binaries homologues of water given in the Nine Series of our table. It follows from what has been said, that the atomic weights of these compounds equals 9+n9. With each $\frac{\text{Sp. Gr.}}{9+n9}=\frac{1}{2}$, therefore Sp. Gr.=4.5+n 4.5. We give below a table of the observed or calculated specific gravities, not only of these compounds, but also of those homologues of NH_3 whose specific gravity has been observed.

Homologues of Water			Homologues of Ammonia Gas		
Sp. Gr. / At. W. = 1/2			Sp. Gr. / At. W. = 1/2		
Sp. Gr. = 4.5 + n4.5			Sp. Gr. = 5.5 + n3		
Symbols	Specific Gravities		Symbols	Specific Gravities	
	Theoret.	Observed		Theoret.	Observed
HO	4.5	9	NH_3	8.5	8.5
HFl	9	....	PH_3	17.25	17.5
HCl	13.5	13.5	AsH_3	39	38.5
HBr	40.5	39.5			
Hl	63	63.5			

As the series of compounds give a greater scope for investigating the relations of properties than is presented by those of the elements, we may expect that these relations will be first discovered in the former, and to my conceptions Chemistry will then have become a perfect science, when all substances have been classed in series of homologues, and when we can make a table which shall contain, not only every known substance, but also every possible one, and when by means of a few general formulas we shall be able to express all the properties of matter, so that when the series of a substance and its place in its series are given, we shall be able to calculate, nay, predict, its properties with absolute certainty; and when our chemical treatises shall have been reduced to tables of homologues, and our laws comprised in a few algebraic formulas, then the dreams of the ancient alchemist will be realized, for the problem of the transmutation of the elements will have been theoretically, if not practically, solved.

THE THREE SERIES

1+n3

Isomorphs					Homologues				Atomic Weights		
6	5	3		1	R	R_2O	RO	RO_2	Theoret.	Observed	n
					Group 1						
MgO,CO_2	$MgO,SO_3+5Aq.$*	.....	.....	.....	Magnesium	.....	MgO	.....	12.5**	12.	n = 8
ZnO,CO_2	$ZnO,SO_3+5Aq.$*	.....	.....	ZnI	Zinc	Zn_2O	ZnO	.....	32.	32.6	n = 21
.....	.....	.....	.....	.....	Cadmium	Cd_2O	CdO	.....	56.	56.	n = 37
					Group 2						
CaO,CO_2	.....	CaO,CO_2	CaO,SO_3	.....	Calcium	.....	CaO	.....	20.	20.	n = 13
.....	.....	SrO,CO_2	SrO,SO_3	.....	Strontium	.....	SrO	SrO_2	44.	43.8	n = 29
.....	.....	BaO,CO_2	BaO,SO_3	.....	Barium	.....	BaO	BaO_2	68.	68.5	n = 45
.....	.....	PbO,CO_2	PbO,SO_3	.....	Lead	Pb_2O	PbO	PbO_2	104.	103.7	n = 69
					Group 3						
.....	.....	.....	.....	.....	Hydrogen	.....	HO	HO_2	1.	1.	n = 0
.....	.....	.....	.....	LiI	Lithium	.....	LiO	.....	6.5	6.5	n = 4
.....	.....	.....	NaO,SO_3	NaI	Sodium	.....	NaO	.....	23.	23.	n = 15
.....	.....	.....	KO,SO_3	KI	Potassium	.....	KO	.....	39.5	39.2	n = 26
.....	.....	.....	AgO,SO_3	AgBr	Silver	Ag_2O	AgO	AgO_2	108.5	108.1	n = 72

AFFILIATIONS

.....	$CuO,SO_3+5Aq.$	.....	.....	.....	Copper	Cu_2O	CuO	CuO_2	32.	31.6	n = 21
MnO,CO_2	$MnO,SO_3+5Aq.$	.....	.....	.....	Manganese	.....	MnO	MnO_2	27.	27.6	n = 18

* Only in isomorphous mixture with $CuO,SO_3+5Aq.$ See Rammelsberg Handbuch der krystallographische Chemie, s. 107.

** The atomic weights of this series must be doubled in order to correspond to 1+n3.

THE FOUR SERIES

4+n4 or 2+n4

Isomorphs			Homologues									Atomic Weights		
4	1			R	R_2O	RO	R_2O_3	RO,R_2O_3	RO_2	RO_3	R_2O_7	RO_4	Theo.	Obs'd	n
				Group 1											
.....	.....	.....	....	Titanium	.....	TiO	Ti_2O_3	.....	TiO_2	.....	.....	.....	26.	25.2	n = 6
.....	.....	.....	Pd	Palladium	.....	PdO	.....	.....	PdO_2	.....	.....	.....	54.	53.3	n = 13
.....	.....	.....	....	Tin	.....	SnO	.....	.....	SnO_2	.....	.....	.....	58.	58.	n = 14
.....	$KCl,SnCl_2$	.....	....	Platinum	.....	PtO	.....	.....	PtO_2	.....	.....	.....	98.	98.5	n = 24
.....	$KCl,PtCl_2$	.....	....	Iridium	.....	IrO	Ir_2O_3	.....	IrO_2	IrO_3	.....	.....	98.	99.	n = 24
.....	$KCl,IrCl_2$	.....	....	Osmium	.....	OsO	Os_2O_3	.....	OsO_2	OsO_3	.....	OsO_4	98.	99.6	n = 24
.....	$KCl,OsCl_2$	.....	....	Gold	.....	AuO	.....	.....	.....	AuO_3	.....	.....	198.	197.	n = 49
.....	.....	.....	Au												
				Group 2											
.....	.....	FeO, Al_2O_3	....	Aluminium	.....	.....	Al_2O_3	.....	.....	.....	.....	.....	14.	13.7	n =3
.....	.....	MgO, Cr_2O_3	....	Chromium	.....	CrO	Cr_2O_3	CrO,Cr_2O_3	CrO_2	CrO_3	Cr_2O_7	.....	26.	26.7	n = 6
.....	.....	MnO, Mn_2O_3	....	Manganese	.....	MnO	Mn_2O_3	MnO,Mn_2O_3	MnO_2	MnO_3	Mn_2O_7	.....	28.	27.6	n = 6
$MnO,SO_3+7Aq.$		ZnO, FeO, Fe_2O_3													
$FeO,SO_3+7Aq.$	.....		Fe	Iron	.....	FeO	Fe_2O_3	FeO,Fe_2O_3	.....	FeO_3	.....	.....	28.	28.	n = 6
$CoO,SO_3+7Aq.$	.....	.....	....	Cobalt	.....	CoO	Co_2O_3	CoO,Co_2O_2	.....	CoO_3	.....	.....	30.	29.5	n = 7
$NiO,SO_3+7Aq.$*	.....	.....	....	Nickel	.....	NiO	Ni_2O_3	NiO,Ni_2O_2	.....	.....	.....	.....	30.	29.6	n = 7
.....	.....	UrO,Ur_2O_3	....	Uranium	.....	UrO	Ur_2O_3	UrO,Ur_2O_3	.....	.....	.....	.....	60.	60.0	n = 14
				Group 3											
$CuO,SO_3+7Aq.$*	.....	.....	Cu.	Copper	Cu_2O	CuO	Cu_2O_3	.....	CuO_2	.....	.....	.....	32.	31.7	n = 7
.....	.....	.....	Hg	Mercury	Hg_2O	HgO	.....	.....	.....	.....	.....	.....	100.	200.	n = 24

* Only in isomorphous mixture with $FeO,SO_3+7Aq.$

THE FIVE SERIES

6 + n5

Isomorphs		Homologues			Atomic Weights		
	1				Theoret.	Observed	n
...	C	Carbon	CO_2	....	6	6	n = 0
...	....	Boron	....	BO_3	11	10.9	n = 1
...	....	Silicon	....	SiO_3	21	21.3	n = 3

AFFILIATIONS

...	Ti	Titanium	TiO_2	....	26	25.2	n = 4

THE SIX SERIES

$8 + n6$

Isomorphs					Homologues					Atomic Weights		
6	3		1							Theoret.	Observed	n
....	.....	.....	.....	.....	Oxygen	.....	.....	.....	.....	8	8	n = 0
....	.....	.....	.....	.....	Nitrogen	NH_2*	NH_3	NO_3	NO_5	14	14	n = 1
....	.....	.....	.....	.....	.....	PMe_2	PMe_3	.....	.....	....	....	.....
....	$NaO, PO_5 + 4HO$	.....	.....	P	Phosphorus	PH_2	PH_3	PO_3	PO_5	32	31	n = 4
....	.....	.....	.....	.....	.....	$AsMe_2$	.....	.....	.....	.....	.....	.....
As	$NaO, AsO_5 + 4HO$	.....	AsO_3	As	Arsenic	.....	AsH_3	AsO_3	AsO_5	74	75	n = 11
....	.....	.....	.....	.....	.....	.....	$SbMe_3$	.....	.....	....	....	.....
Sb	.....	SbS_3	SbO_3	.....	Antimony	.....	SbH_3	SbO_3	SbO_5	128	129	n = 20
....	.....	.....	.....	.....	.....	.....	$BiAe_3$	.....	.....	....	....	.....
Bi	.....	BiS_3	.....	.....	Bismuth	.....	BiH_3	BiO_3	BiO_5	206	208	n = 33

AFFILIATIONS

....	.....	.....	.....	.....	Chromium	.....	.....	CrO_3	.....	26	26 7	n = 3
....	.....	.....	.....	.....	Vanadium	.....	.....	VaO_3	.....	68	68.5	n = 10

* Only known in combination.

THE EIGHT SERIES

8 + n8 or 4 + n8

Isomorphs							Homologues					Atomic Weights		
6	4	3		2	1							Theoret.	Observed	n
..	.....	.....	.....	.....	†PbO	*Cu_2O	Oxygen	HO	HO_2	.....	.....	8	8	n = 0
..	.....	KO,SO_3	‡Fe_2S_4	.....	PbS	*Cu_2O	Sulphur	HS	HS_2	SO_2	SO_3	16	16	n = 1
..	.....	KO,SeO_3	.....	.....	PbSe	....	Selenium	HSe	...	SeO_2	SeO_3	40	39.6B	n = 4
..	.....	.....	.....	PbO,MoO_3	....	....	Molybdenum	...	...	MoO_2	MoO_3	44	46	n = 5
Te	.....	.....	.....	.....	PbTe	....	Tellurium	HTe	...	TeO_2	TeO_3	64	64.1B	n = 7
..	.....	.....	.....	.....	....	....	Vanadium	...	...	VaO_2	VaO_3	68	68.5B	n = 8
..	FeO,WO_3	.....	.....	PbO,WO_3	....	....	Tungsten	...	...	WO_2	WO_3	92	92	n = 11
..	FeO,TaO_3	.....	.....	.....	....	....	Tantalum	...	...	TaO_2	TaO_3	188	184	n = 23

Affiliations

As	.....	$Fe_2(AsS_2)$	.....	.....	....	....	Arsenic	...	...	.....	.....	148	150	n = 18
..	KO,MnO_3	.....	.....	.....	....	....	Manganese	...	...	.....	MnO_3	28	27.6	n = 3

* Mitscherlich J. pr. Chem. 19,449. † Becquerel Ann. Chim. Phys. 51,105. ‡ G. Rose Krystallochemische Mineralsystem.

THE NINE SERIES

8 + n9

Isomorphs				Homologues							Atomic Weights		
3		1									Theoret.	Observed	n
.....	.....	*Cu_2O	.....	Oxygen	HO	.....	.....	.....	.....	.....	8	8	.....
.....	.....	.....	KFl	Fluorine	HFl	.....	.....	.....	.....	.....	17	18.8B	n = 1
.....	.....	.....	KCy	Cyanogen	HCy	CyO	.....	.....	.....	.....	26	.....	n = 2
KO,ClO_7	.....	*Cu_2Cl	KCl	Chlorine	HCl	ClO	ClO_3	ClO_4	ClO_5	ClO_7	35	35.5	n = 3
.....	.....	.....	KBr	Bromine	HBr	BrO	.....	.....	BrO_5	.....	80	80	n = 8
.....	.....	.....	KI	Iodine	HI	IO	.....	.....	IO_5	IO_7	125.	126.9M	n = 13

Affiliations

.....	KO,CrO_3	.....	.....	Chromium	.....	.....	.....	.....	.....	Cr_2O_7	53	53.4	n = 5
KO,Mn_2O_7	KO,MnO_3	.....	.....	Manganese	.....	.....	.....	.....	.....	Mn_2O_7	53	55.2	n = 5
.....	.....	.....	.....	Osmium	.....	.....	.....	OsO_4	.....	.....	98	99.4	n = 10
.....	.....	.....	.....	Gold	.....	.....	AuO_5	.....	AuO_5	.....	197	197	n = 21

* Mitscherlich J. pr. Chem. 19,449.

EXPLANATION OF THE TABLE

The formula at the head of each series is a general expression for the atomic weights of that series. The names of the series are derived from the "Common Differences," which are the numbers multiplied by n in the general formulas. In the columns headed "Theoretical" are given the atomic weights calculated from these formulas and the values of n given in the last columns at the right of each division of the table. In the columns headed "Observed" will be found the observed values of the same atomic weights. These have been taken from the table of atomic weights given in the last volume of Liebig and Kopp's *Jahresbericht* (for 1852), with the exception of those against which are placed the initials of the observers. The last were taken from Weber's Atomgewichts Tabellen. In some cases the atomic weight is taken at twice its received values, but it is then underlined. The compounds in any one column at the right of the names of the elements are homologous. In the same way, those in any one at the left are isomorphous. The numbers at the head of these last columns indicate crystalline systems as follows: 1. Monometric; 2. Dimetric; 3. Trimetric; 4. Monoclinic; 5. Triclinic; 6. Rhombic. The data from which the table was compiled were drawn from numerous sources, but especially from the following works: Gmelin's Handbook of Chemistry, Graham's "Elements of Chemistry," Phillips's "Mineralogy by Brooke and Miller," and Gustav Rose's "Krystallo-chemische Mineralsystem." References

have been given only in a few cases, to avoid crowding the tables. For authorities in other cases, the author would refer to the above-mentioned works.

In the recent development of chemistry along physical lines, America has played quite a prominent part. Indeed, the contributions of J. Willard Gibbs (1839-1903) are fundamental in nature and of broad application. He was born in New Haven, Connecticut. He graduated from Yale in 1858, where he became (1871) professor of mathematical physics. The following admirable account of his studies relating to pure chemistry is from the pen of Chas. S. Hastings (Biographical Memoirs of The National Academy of Sciences, VI, 373): "In 1873 he published his first papers, the first being entitled 'Graphical Methods in the Thermodynamics of Fluids' and the second 'A Method of Geometrical Representation of the Thermodynamic Properties of Substances by Means of Surfaces.' These were followed in 1876 and 1878 by the two parts of the great paper 'On the Equilibrium of Heterogeneous Substances,' which is generally, and probably rightly, considered his most important contribution to physical science, and which is unquestionably among the greatest and most enduring monuments of the wonderful scientific activity of the nineteenth century. The first two papers of this series, although somewhat overshadowed by the third, are themselves very remarkable and valuable contributions to the theory of thermodynamics; they have proved useful and fertile in many direct ways and, in addition, it is difficult to see how, without them, the third could have been written. In logical development the three are very closely connected, and meth-

ods first brought forward in the earlier papers are used continually in the third.

Willard Gibbs was much inclined to the use of geometrical illustrations, which he employed as symbols and aids to the imagination, rather than the mechanical models which have served so many great investigators; such models are seldom in complete correspondence with phenomena they represent, and Gibbs' tendency toward rigorous logic was such that the discrepancies apparently destroyed for him the usefulness of the model. Accordingly he usually had recourse to the geometrical representation of his equations, and this method he used with great ease and power. With this inclination, it is probable that he made much use, in his study of thermodynamics, of the volume-pressure diagram, the only one which, up to that time, had been used extensively. To those who are acquainted with the completeness of his investigation on any subject which interested him, it is not surprising that his first published paper should have been a careful study of all the different diagrams which seemed to have any chance of being useful. Of the new diagrams which he first described in this paper, the simplest, in some respects, is that in which entropy and temperature are taken as co-ordinates; in this as in the familiar volume-pressure diagram, the work or heat of any cycle is proportional to its area in any part of the plane; for many purposes it is far more perspicuous than the older diagram, and it has found most important practical applications in the study of the steam engine. The diagram, however, to which he gave most attention was the volume-entropy diagram, which presents many advantages when the properties of bodies are to be studied, rather than the work they do or the heat they give out. The chief reason for this superiority is that volume and entropy are both proportional to the quantity of substance, while pressure and temperature are not; the repre-

J. Willard Gibbs

sentation of coexistent states is thus especially clear, and for many purposes the gain in this direction more than counterbalances the loss due to the variability of the scale of work and heat. No diagram of constant scale can, for example, adequately represent the triple state where solid, liquid, and vapor are all present; nor, without confusion, can it represent the states of a substance which, like water, has a maximum density; in these and in many other cases the volume-entropy diagram is superior in distinctness and convenience.

In the second paper the consideration of graphical methods in thermodynamics was extended to diagrams in three dimensions. James Thomson had already made this extension to the volume-pressure diagram by erecting the temperature as the third co-ordinate, these three immediately cognizable quantities giving a surface whose interpretation is most simple from elementary considerations, but which, for several reasons, is far less convenient and fertile of results than one in which the co-ordinates are thermodynamic quantities less directly known. In fact, if the general relation between the volume, entropy and energy of any body is known, the relation between the volume, pressure, and temperature may be immediately deduced by differentiation; but the converse is not true, and thus a knowledge of the former relation gives more complete information of the properties of a substance than a knowledge of the latter. Accordingly Gibbs chooses as the three coordinates the volume, entropy, and energy, and, in a masterly manner, proceeds to develop the properties of the resulting surface, the geometrical conditions for equilibrium, the criteria for its stability or instability, the conditions for coexistent states and for the critical state; and he points out, in several examples, the great power of this method for the solution of thermodynamic problems. The exceptional importance and beauty of this work by a hitherto unknown writer was immediately recognized by

Maxwell, who, in the last years of this life, spent considerable time in carefully constructing, with his own hands, a model of this surface, a cast of which, very shortly before his death, he sent to Gibbs.

One property of this three dimensional diagram (analogous to that mentioned in the case of the plane volume-entropy diagram) proved to be of capital importance in the development of Gibbs' future work in thermodynamics, the volume, entropy, and energy of a mixture of portions of a substance in different states (whether in equilibrium or not) are the sums of the volumes, entropies, and energies of the separate parts, and, in the diagram, the mixture is represented by a single point which may be found from the separate points, representing the different portions, by a process like that of finding centers of gravity. In general this point is not in the surface representing the stable states of the substance, but within the solid bounded by this surface, and its distance from the surface, taken parallel to the axis of energy, represents the available energy of the mixture. This possibility of representing the properties of mixtures of different states of the same substance immediately suggested that mixtures of substances differing in chemical composition, as well as in physical state, might be treated in a similar manner; in a note at the end of the second paper, the author clearly indicates the possibility of doing so, and there can be little doubt that this was the path by which he approached the task of investigating the conditions of chemical equilibrium, a task which he was destined to achieve in such a magnificent manner and with such advantage to physical science.

In the discussion of chemically homogeneous substances in the first two papers, frequent use had been made of the principle that such a substance will be in equilibrium if, when its energy is kept constant, its entropy cannot increase; at the head of the third paper the author puts the famous statement of Clausius: "Die Energie der Welt ist

constant. Die Entropie der Welt strebt einem Maximum zu." He proceeds to show that the above condition for equilibrium, derived from the two laws of thermodynamics, is of universal application, carefully removing one restriction after another, the first to go being that the substance shall be chemically homogeneous. The important analytical step is taken of introducing, as variables in the fundamental differential equation, the masses of the constituents of the heterogeneous body; the differential coefficients of the energy with respect to these masses are shown to enter the conditions of equilibrium in a manner entirely analogous to the "intensities," pressure and temperature, and these coefficients are called potentials. Constant use is made of the analogies with the equations for homogeneous substances, and the analytical processes are like those which a geometer would use in extending to n-dimension the geometry of three.

It is quite out of the question to give, in brief compass, anything approaching an adequate outline of this remarkable work. It is universally recognized that its publication was an event of the first importance in the history of chemistry, that in fact it founded a new department of chemical science which, in the words of Le Chatelier, is becoming comparable in importance with that created by Lavoisier. Nevertheless it was a number of years before its value was generally known; this delay was due largely to the fact that its mathematical form and rigorous deductive processes made it difficult reading for anyone, and especially so for students of experimental chemistry, whom it most concerns; twenty-five years ago there was relatively only a small number of chemists who possessed sufficient mathematical knowledge to read easily even the simpler portions of the paper. Thus it came about that a number of natural laws of great importance which were, for the first time, clearly stated in this paper were subsequently, during its period of neglect, discovered by others, some-

times from theoretical considerations, but more often by experiment. At the present time, however, the great value of its methods and results is fully recognized by all students of physical chemistry. It was translated into German in 1891 by Ostwald and into French in 1899 by Le Chatelier; and, although so many years had passed since its original publication, in both cases the distinguished translators give, as their principal reason for undertaking the task, not the historical interest of the memoir, but the many important questions which it discusses and which have not even yet been worked out experimentally. Many of its theorems have already served as starting points or guides for experimental researchers of fundamental consequence; others, such as that which goes under the name of the "Phase Rule," have served to classify and explain, in a simple and logical manner, experimental facts of much apparent complexity; while still others, such as the theories of catalysis, of solid solutions, and of the action of semi-permeable diaphragms and osmotic pressure, showed that many facts, which had previously seemed mysterious and scarcely capable of explanation, are in fact simple, direct and necessary consequences of the fundamental laws of thermodynamics. In the discussion of mixtures in which some of the components are present only in very small quantity (of which the most interesting cases at present are dilute solutions) the theory is carried as far as possible from *a priori* considerations; at the time the paper was written the lack of experimental facts did not permit the statement, in all its generality, of the celebrated law which was afterward discovered by van't Hoff; but the law is distinctly stated for solutions of gases as a direct consequence of Henry's law and, while the facts at the author's disposal did not permit a further extension, he remarks that there are many indications "that the law expressed by these equations has a very general application."

It is not surprising that a work containing results of

such consequence should have excited the profoundest admiration among the students of the physical sciences; but even more remarkable than the results, and perhaps of even greater service to science, are the methods by which they were attained; these do not depend upon special hypotheses as to the constitution of matter or any similar assumption, but the whole system rests directly upon the truth of certain experimental laws which possess a very high degree of probability. To have obtained the results embodied in these papers in any manner would have been a great achievement; that they were reached by a method of such logical austerity is a still greater cause for wonder and admiration. And it gives to the work a degree of certainty and an assurance of permanence, in form and matter, which is not often found in investigations so original in character.

Willard Gibbs was a member among others of the Connecticut Academy of Arts and Sciences, the National Academy of Sciences, the American Philosophical Society, the Dutch Society of Sciences, Haarlem; the Royal Society of Sciences, Goettingen; the Royal Institution of Great Britain; the Cambridge Philosophical Society, the London Mathematical Society, the Manchester Literary and Philosophical Society, the Royal Academy of Amsterdam, the Royal Society of London, the Royal Prussian Academy of Berlin, the French Institute, the Physical Society of London, and the Bavarian Academy of Sciences, and the recipient of many academic honors. In 1881 he received the Rumford Medal from the American Academy of Boston, and in 1901 the Copley Medal from the Royal Society of London.

The preceding pages contain the most significant efforts of American chemists, extending over a period of a little more than one hundred years. The results compare favorably with those of a like period in the early history of

our science in any other country. The earliest contributions represented beginnings. They were carried out when the republic was young. Years of preparation and adjustment followed for the new nation, and then years of internal strife; and it is exceedingly gratifying and encouraging to observe that the scientists of the country, including the noble guild of chemists, contributed much to bring the national resources into prominence and usefulness, as well as to guide the educational development of the States.

To the *Journal of Science,* founded by Silliman, were added *The American Chemist* (1870—by Charles F. Chandler), the *American Chemical Journal* (1879—by Ira Remsen), the *Journal of Analytical and Applied Chemistry* (1887, by Edward Hart), the *Journal of Physical Chemistry* (1896, by Wilder D. Bancroft), the *Chemical Engineer* (1904, by R. K. Meade), the Transactions of the American Electro-Chemical Society, the Transactions of the Institute of Chemical Engineers, and the most potent factor in chemical affairs—the American Chemical Society, with its admirable journals. All these have fostered the growth and development of chemistry in this country.

The maintenance of well equipped laboratories in the colleges, as well as the very palatial structures connected with some of the older universities and with the larger state universities, are further evidence of our development along chemical lines.

It is not the writer's purpose to discuss the investigations which have come from the many working centers of the United States during recent years, that story awaits another narrator; but, if only a desire, on the part of

Americans to learn more concerning the place which American chemists occupy in the world's history of chemistry, is awakened, this compilation of facts will not only have been a pleasure but it will have served a worthy purpose.

INDEX

Acetite of lead, 67.
Affinity, 55.
Agricultural chemistry, 231-235.
Air in charcoal, 91.
Alchemical notions, 21.
Alumine, 63.
Aluminium, atomic weight of, 225.
American Philosophical Society, 1.
Animal chemistry, 69.
Antiphlogistic system, the, answer to Priestley's arguments against, 80.
Apparatus for deflagrating carburets, etc., 201-205.
Arabians, 18, 20, 21, 23.
Atmosphere, 56.
Attributes of matter, 54.

Bache, Alexander Dallas, 230.
Bacon, Francis, 25.
Bacon, Roger, 22.
Bailey, J. W., 226.
Balance, use of, 11.
Becher, 27, 29, 34.
Beck, Lewis C., 226.
Blowpipe, hydrostatic, 165.
Blowpipe, memoir upon, by Hare, 153, 178.
Boerhaave, 23.
Booth, James C., 245-246.
Bowen, George T., 222.
Boye, Martin, 236.
Boyle, 29.
Bruce, Archibald, 219.

Calcination of a metal in pure air, 83.
Calorimotor, 190.
Carbonic acid, 85.
Cast steel, 62.
Chalybeate waters, analysis of, 2-4.
Charcoal, air in, 91.
Chemical Society of Philadelphia, 12, 15, 44.
  advertisements of, 45-48.
  annual oration of, 13, 49, 51.
  junior members of, 12.
  officers of, 44.
Chemistry, 18.
  publications devoted to, 350.
  sketch of revolutions in, 17.
  vegetable, 63.
Chloroform, discovery of, by Guthrie, 226, 229.
Chymical analysis, 2-3.
Cleaveland, Parker, 221.
Clemson, Thomas G., 219.
Cloud, James, 221.
Columbian Chemical Society, 208-209.
  contents of memoir of, 213-214.

Columbian Chemical Society, history of, 210.
officers and members of, 215-218.
Columbian mineral, 41.
Compound blowpipe, 153.
invention of, 152.
Cooke, Josiah Parsons, 301-304.
books and researches of, 305-306.
on the numerical relations between the atomic weights of the elements, 307, 342.
Cooper, Thomas, 128, 137, 140-144, 146.
Cooper, letter of, to Dr. Manners, 129-134.
Cooper, professor of chemistry, 137, 139.
Cooper and government, 138-139.
Coxe, John Redman, 223.
and electric telegraphy, 223.
Cutbush, James S., 223-224.

Dana, James Freeman, 221.
Dana, Samuel Latham, 222.
Deflagrating carburets, apparatus for, 201-205.
Deflagrator, 188-189.
De Normandie, Dr. John, 2-3.
Dexter, Aaron, 148.

Emery, 255.
Emmett, J. P., 224.

Finery cinder, 88.
Fixed air, 85.

Galvanism and Hare, 194.
Geber, 23.
Genth, Frederick A., 261-263.
Gibbs, J. Willard, 341-3, 349.
and chemically homogeneous substances, 346.
geometrical illustrations of, 344.
graphical methods of, 343, 345.
phase rule, 348.
thermodynamics of, 347.
Gibbs, Wolcott, 264-271.
Glauber, 26.
Glauber's salts, 26.
Glucina in chrysoberyl, 151.
Gorham, John, 221.
Griscom, John, 224.
Guthrie, Samuel, 226.
and chloroform, 226, 229.

Hales, 29.
Hare, Robert, 137, 152, 186, 191, 192, 205.
account of, by Silliman, 179, 183.
calorimotor of, 190.
compound or hydrostatic blowpipe of, 153, 178, 183-187.
deflagrator of, 188-189.
and galvanism, 194.
letter of, to Silliman, 192.
Hunt, T. Sterry, 246-252.
Hutchinson, James, 148.

Inflammable air, 87.
Isolation of calcium, 195, 201.

Johnson, Samuel W., 275-276.
Johnson, "How Crops Feed," by, 275.
"How Crops Grow," by, 275.

Kircher, Father, 26.

Lavoisier, 12, 20, 30-33.
Lea, M. Carey, 277, 301.
on allotropic silver, 295-296.
on chemistry of photography, 282-294.
on classification of the elements, 298-300.
on endothermic reactions, 298.
on ethylamine, 280.
on gelatine, 281.
on numerical relations between elementary bodies, 277.
the platinum group and, 278.
relative affinity of acids, 279.
starch test for iodine, 278.
on urea, 280.
Light, 56.

McCauslin, Albert, Dr., 7.
McNevin, William, Dr., 224.
Maclean, John, 92, 97, 147-148.
Maclean, John, letter from Woodhouse to, 92.
Madison, J., 5.
Mallet, John W., 276.
determination of atomic weights by, 276-277.
Mather, W. W., 225.
and atomic weight of aluminium, 225.
Matter, attributes of, 54.
Mayow, 28.
Medicine, 69.
Mineralogy, 58.
Mitchill, Samuel Latham, 148-149.

Niagara Falls, 7.
Niter, 60.
Norton, John P., 231.

Observations on methods of obtaining oxygenous gas, 97-100, 101, 102.
Olmstead, Denison, 223.
Oxygenated muriate of potash, 61.

Paracelsus, 23-24.
Pascalis, Felix, 49.
Pathology, 71.
Peter the Hermit, 20
Pharmacy, 73.
Philadelphia, Chemical Society of, 12, 13, 15.
Philadelphia Laboratory, 15.
Philosophers' stone, 21.
Phlogiston, 27.
Phosphorus, 72.
Physiology, 71.
Potassium, apparatus for isolation of, 135-136.
discovery of, 129.
obtained by Woodhouse, 134.
Precipitation of one metal by another, 89-90.
Prescott, Albert B., 272-274.
Priestley, 20, 29-30, 35, 76, 82-87, 91-93, 95-97, 103, 147.

Priestley, account of, by Silliman, 118-120.
arguments of, 80.
description of, by Caldwell, 144-145.
letter of, to Dr. Mitchill, 112-118.
to Rush, 110.
letter to, from Prof. Rush, 109.
in the minutes of Trustees of University of Pennsylvania, 111-112.
obituary notices of, 121-127.
Production of potassium by Cooper, 128.
Publications devoted to chemistry, 350.
Pugh, Evan, 231.
and agricultural chemistry, 231, 235.

Revolutions in chemistry, 17.
notes on, 36, 41.
Rittenhouse, 5.
Rogers Brothers, 135.
Rogers, Henry D., 236.
Rogers, James B., 235.
Rogers, Robert E., 236-241.
Rogers, William B., 236.

Seybert, Adam, 150.
analysis of air by, 150.
Seybert, Henry, 151.
Silliman, Benjamin, 103, 192, 206-208.
Silliman, account of Hare by, 179, 183.
on Hare's blowpipe, 179.
Smith, John Lawrence, 252-260.
Smith, Thomas P., 15, 41, 42, 44.
Society, Chemical, of Philadelphia, 12.
Spray of the falls, 7.
Stahl, 27, 29, 34.
Sweet springs, experiments upon, 5.

Torrey, John, 220-221.
Troost, Gerard, 222.

Van Helmont, 24.
Vanuxem, Lardner, 220.

Woodhouse, James, 12, 75-76, 91-92, 97, 104, 106-108, 148.
and the metal potassium, 134.
answer of, to Priestley, 80.
letter of, to John Maclean, 92.
opinion of, on Benjamin Rush, 106.
word-picture of, by Silliman, 103-106.
Wetherill, Charles M., 235.
Wormley, Theodore G., 242-244.

"Young Chemist's Pocket Companion," 77-79.

QD
18
U6
S6
Smith, Edgar Fahs.
Chemistry in America.
529

QD
18
U6
S6
529

THE VINING LIBRARY
W.V.I.T.
MONTGOMERY, W. VA. 25136

BRODART PRINTED IN U.S.A.

The Irving Libr
INSTITUTE OF TECHNO